VAN NOSTRAND'S

ECLECTIC

ENGINEERING MAGAZINE.

NO. XC.—JUNE, 1876.—VOL. XIV.

CALCULATIONS OF STRAINS IN ARCH BRIDGES.*

By CHARLES PFEIFER, C. E.

RESISTANCES OF ABUTMENTS AND DEFLECTIONS OF ARCH PRODUCED BY MOVABLE LOADS.†

Let A C A_1, Fig. 1, be the centre line of a rib of an upright arch bridge; O its center; A A_1 the span of the center line, C D the versed sine. Let the movable load be supposed to extend from A to any point B, and to be equally distributed horizontally. Let F be any point of the center line. Let A be the origin of rectangular coordinates, A A_1 being the axis of x, and put

x, y=the coordinates of F before flexure,
x_1, y_1=the coordinates of F after flexure,
δ=C O F,
β=C O B,
$\propto$=C O A,
R=the radius of the center line,
$2a$=the span A A_1,
h=C D,
na=the horizontal distance of B from A,
q=the load on a rib per unit of length (measured horizontally),

then, putting also $x=ma$, we have the relations

$$1\left\{\begin{array}{l} x=ma=\mathrm{R}\ (\sin\propto-\sin\delta), \\ y=\mathrm{R}\ (\cos.\ \delta-\cos.\propto), \\ na=\mathrm{R}\ (\sin\propto-\sin\beta), \end{array}\right.$$

whence, since $a=\mathrm{R}\sin\propto$, we have for computing the values of δ and β for any given values of m and n,

$$2\left\{\begin{array}{l} \sin\delta=(1-m)\sin\propto, \\ \sin\beta=(1-n)\sin\propto, \end{array}\right.$$

The reactions caused by the load qna at the abutments A and A_1 may be represented by their resultants, single forces T and T_1, intersecting the radial planes through the ends of the arch at the points G and H, whose distances from A and A_1, measured towards the center O, are b and b_1. If the points G or H should be above A or A_1, b or b_1 would be negative.

The forces T and T_1 may be replaced by their vertical and horizontal components P, Q and P_1, Q_1.

As the forces acting on the arch must be in equilibrium, we have the following equations:

* A paper prepared for the Engineers' Club of St. Louis, Mo.

† A great part of the contents of this paper has already been published in the appendix to the report of the Chief Engineer of the Illinois and St. Louis Bridge of 1868, containing the calculations for this bridge. The arrangement of these calculations for publication was intrusted by the Chief Engineer, Capt. James B. Eads, to the late W. Chauvenet, LL. D., Chancellor of Washington University, formerly Professor of Mathematics in the U. S. Naval Academy at Annapolis. As I could not expect to improve on his arrangement, I took the liberty of copying from it whatever I could use, considering the wider scope of this present paper.

3. $P_1 = -P + q\,n\,a$ (sum of vertical forces = 0),

4. $Q_1 = Q$ (sum of horizontal forces = 0),

5. $b_1 (P_1 \text{ sine } \alpha + Q_1 \text{ cos. } \alpha) = b (P \text{ sine } \alpha + Q \text{ cos. } \alpha) - 2\,Pa + 2\,q\,n\,a^2 \left(1 - \frac{n}{4}\right)$ (sum of moments around $A_1 = 0$).

The quantities $b(P \text{ sine } \alpha + Q \text{ cos. } \alpha)$ and $b_1 (P_1 \text{ sine } \alpha + Q_1 \text{ cos. } \alpha)$ are the bending moments at the abutments.

The equations 3, 4 and 5, which exhaust the conditions of *equilibrium* for the whole arch, determine the resistance of the abutment on the right side when those on the left side are known, but they give us no means to determine the latter. To obtain these we must consider the resistance which the abutments offer to the *deflection* of the arch.

This resistance depends

1st. Upon the construction of the abutments and the peculiar manner in which the ends of the arch are connected with them;

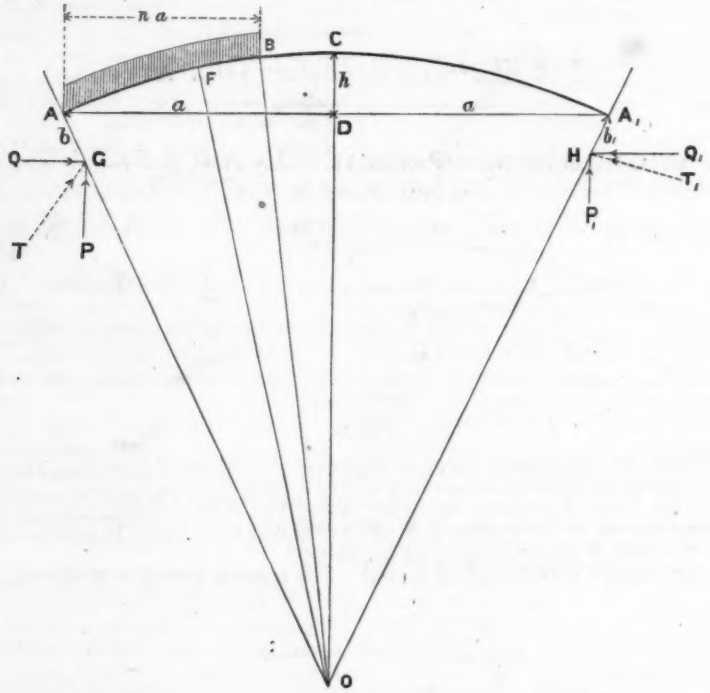

FIG. 1.

2d. Upon the properties of the arch itself, its form, cross-section, method of construction, etc.

For illustration, let us assume that the arch at A and A_1 end in pins, which rest on stools of which one is mounted on rollers as is done in truss bridges to avoid strains from expansion or contraction by changes of temperature.

It is evident that in this case the resultant of the resistance at each abutment must pass through the center of the end-pin, hence

$$b = b_1 = 0,$$

which makes also the bending moments at the abutments = 0.

On account of the rollers the abutments can offer no horizontal resistance, hence

$$Q = Q_1 = 0.$$

The vertical resistances of the abut-

ments may then be obtained by 5 and 3, viz.:

$$P=q\,n\,a\left(1-\frac{n}{4}\right), \text{ and } P_1=q\frac{n^2 a}{4}$$

The arch is in this case but a girder with a high camber.

Let us now assume that the stools on which the end-pins A and A_1 of the arch rest, be firmly set into rigid abutments, but that the arch be constructed with a hinge in its crown at C, which prevents a bending moment at this point.

We have then again, as in the former case,

$$b=b_1=0,$$

$$P=q\,n\,a\left(1-\frac{n}{4}\right),$$

$$P_1=q\frac{n^2 a}{4},$$

but we can no longer put the horizontal components, Q and Q_1, = 0, and have to determine them by the condition that the forces acting on the right half of the arch must have no tendency to turn it about the hinge at C. This condition is expressed by the equation

$$Q_1 h-P_1 a=0,$$

which, with the value of P_1 found above, and by equation 4 gives

$$Q=Q_1=q\frac{n^2 a^2}{4h}.$$

In the cases just considered the determination of P, Q and b (P sine $\propto$ + Q cos. $\propto$) from the peculiarities of the construction was very simple, but when there is no hinge in the center, or no pivots at the ends, it becomes necessary to consider the deflection of the arch in order to arrive at the proper values for the reactions of the abutments.

If A O B, Fig. 2, is the center-line of an elastic arch, any normal section of which, as H N, is a symmetrical figure

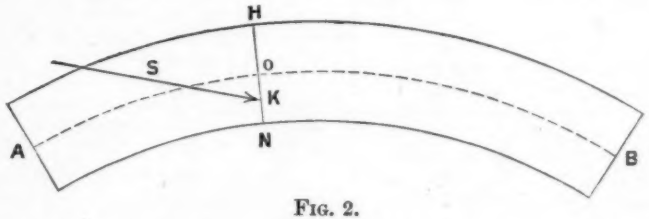

Fig. 2.

with respect to the center line, and if different external forces are acting in the plane of the center line, then the internal forces acting in the section H N on the portion A O of the arch have to be in equilibrium with all the external forces acting on the same portion. These external forces may be supposed to be first reduced to their resultant, a single force S acting at some point K in the normal plane.

The force S acting at K may then be

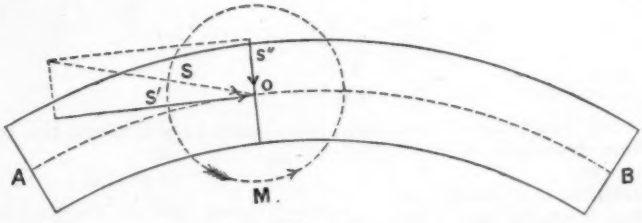

Fig. 3.

replaced by an equal force S (Fig. 3) acting at the center O of the normal section, and a couple of forces whose moment M tends to produce rotation about the point O.

Finally, the force S may be replaced by two components S′ and S″, one acting at O perpendicular to the normal plane, that is, in the direction of the arch at this point; the other acting in the nor-

mal plane, that is, in the direction of the normal to the arch at O. The force S′ produces a *compressive* or *tensile strain* in the direction of the arch; the force S″ the *shearing strain.*

The force S′, not exceeding the limit of elasticity of the material, a proper sectional area of the arch being furnished, has no tendency to change the curvature of the arch, and produce only a small variation in the length of the center line which at present we neglect.*

As we know further from experience that in an arch whose vertical thickness is small compared with its radius and with its length, and whose strains do not exceed the limits of elasticity, a plane section H N normal to the curve of the arch before flexure remains normal to the curve of the deflected arch, which could not be the case if the deflection was in any material degree influenced by the shearing strains S″, we may for all practical purposes consider the moment M as the sole agent in changing the curve of the arch.

Omitting therefore for the present the forces S′ and S″, we shall consider first the effect of the moment M at the several points of this arch.

The moment M produces strains of the same absolute value above and below the neutral axis, compressive on one side, and tensile on the other side of the axis. There results a deflection of the centre line, and hence M is called the *moment of flexure.*

If we put

δ = the angle which the normal at O makes with a vertical line *before* flexure,

δ_1 = the angle which the normal at O makes with a vertical line *after* flexure,

R = the radius of curvature *before* flexure,

R_1 = the radius of curvature *after* flexure,

s = the arc A O,

ds = the infinitesimal element of the arc s,

θ = the moment of inertia of the normal section of the arch, the condition that the length of the center line, or arc A O, is not changed by the flexure, gives

$$6.\quad ds = -R\,d\delta = -R_1\,d\delta_1.$$

The arch being conceived to be composed of fibers of infinitesimal thickness lying in the direction of the curve, the position of any fiber may be expressed by coordinates u and v, the axis of u being a horizontal line through O perpendicular to the vertical plane of the arch, and the axis of v being the normal line at O, or intersection of the normal and vertical planes at this point.

We shall assume that the axis of u is the *neutral axis.*† The condition that the normal section H N remains normal to the curve A O at O, during flexure, requires that fibers at different distances from the neutral axis should be compressed or elongated by quantities proportional to those distances, that is, proportional to the values of v. Conceiving, therefore, two consecutive normal planes to be drawn, the length of the infinitesimal portion, or element of any fibre intercepted between these two planes, will be before flexure $ds + v\,d\delta$, and after flexure $ds + v\,d\delta_1$; the element has therefore been increased in length by $(d\delta_1 - d\delta)\,v$. If then

ε = the modulus of elasticity of the material composing the arch,

$du\,dv$ will be the area of the section of the fiber, and $\varepsilon.\,du\,dv$ will be the force necessary to elongate the element $ds + v\,d\delta$ by its own length; and hence the force necessary to elongate it by the quantity $(d\delta_1 - d\delta)\,v$ will be

$$\frac{(d\delta_1 - d\delta)\,v}{ds + v\,d\delta} \cdot \varepsilon.\,du,\,dv,$$

or substituting $R\,d\delta$ for ds,

$$\frac{d\delta_1 - d\delta}{d\delta} \cdot \varepsilon.\ \frac{du\,dv\,v}{R+v}$$

And the moment of this force about the neutral axis is the force multiplied by v, *i. e.*,

$$\frac{d\delta_1 - d\delta}{d\delta} \cdot \varepsilon.\ \frac{du\,dv\,v^2}{R+v}$$

The total moment M of all the fibers is, therefore,

$$M = \frac{d\delta_1 - d\delta}{d\delta} \cdot \varepsilon. \int\int \frac{du\,dv\,v^2}{R+v},$$

* The influence of this force will be considered afterwards. See page 495.

† For a straight prismatic beam this assumption is exact, and it is sufficiently correct for a curved beam whose radius is very great in comparison with its thickness.

the limits of the integrals being determined by the form of the normal section.

The figure of the normal section being symmetrical with respect to the neutral axis, the values of $\frac{du\,dv\,v^2}{R+v}$ at equal distances, above and below this axis, will be

$$\frac{dv\,dv\,v^2}{R+v} \text{ and } \frac{du\,dv\,v^2}{R-v}$$

whose sum is

$$\frac{2}{R}\cdot\frac{du\,dv\,v^2}{1-\frac{v^2}{R^2}}.$$

As it is assumed that the vertical thickness of the arch is small compared with the radius, $\frac{v^2}{R^2}$ will be the square of a small fraction, hence no appreciable error is committed in taking the integral by omitting this fraction,* and we may take

$$M=\frac{d\delta_1-d\delta}{d\delta}\cdot\frac{\varepsilon}{R}\cdot\int\int du\,dv\,v^2$$

The integral $\int\int du\,dv\,v^2$ is simply the *moment of inertia* of the normal section of the arch, and by substituting for it the letter θ, we obtain the equation

$$7.\quad d\delta_1-d\delta=\frac{R\,M}{\varepsilon\,\theta}\,d\delta.$$

In applying this equation to our arch, we shall assume θ as constant.†

As the internal forces in any section F, considered as acting on the portion A F of the arch, have to be in equilibrium with the external forces acting on the same portion of the arch, the moment M of the internal forces around F must be equal to the moment of the external forces for the same point. This gives us the equations

a/ for loaded part, from $x=o$ to $x=n\,a$,

$$M=q\frac{x^2}{2}-P(x-b\sin.\alpha)+Q\,(y+b\cos.\alpha)$$

or by 1, and by introducing

$$8.\quad C=\frac{b}{R}$$

$$9a\quad M=-PR\,[(1-c)\sin.\alpha-\sin.\delta]+QR\,[\cos.\delta-(1-c)\cos.\alpha]+\frac{qR^2}{2}\,(\sin.\alpha-\sin.\delta)^2\;;$$

b/ for unloaded part, from $x=n\,a$ to $x=2a$.

$$M=-P\,(x-b\sin.\alpha)+Q\,(y+b\cos.\alpha)-q\,n\,a\left(x-\frac{n\,a}{2}\right)$$

or by 1 and 8,

$$9b.\quad M=-PR\,[(1-c)\sin.\alpha-\sin.\delta]+QR\,[\cos.\delta-(1-c)\cos.\alpha]-qR^2\sin.\delta\,(\sin.\alpha-\sin.\beta)+\frac{qR^2}{2}\,(\sin.^2\alpha-\sin.^2\beta).$$

Substituting these values of M in equation 7 and integrating,* we obtain

a/ for loaded part,

$$10a.\quad \delta_1-\delta=\frac{R^2}{\varepsilon\,\theta}\left(\left\{-P(1-c)\sin.\alpha-Q(1-c)\cos.\alpha+\frac{qR}{2}\sin.^2\alpha+\frac{qR}{4}\right\}\delta-(P-qa)\cos.\delta+Q\sin.\delta-\frac{qR}{4}\sin.\delta\cos.\delta+A\right)$$

b/ for unloaded part,

$$10b.\quad \delta_1-\delta=\frac{R^2}{\varepsilon\,\theta}\left(\left\{-P\,(1-c)\sin.\alpha-Q(1-c)\cos.\alpha+\frac{qR}{2}\sin.^2\alpha-\frac{qR}{2}\sin.^2\beta\right\}\delta-(P-qa+qR\sin.\beta)\cos.\delta+Q\sin.\delta+B\right)$$

in which A and B are constants of integration.

For $\delta=\beta$ the equations (10*a*) and (10*b*) must be identical, which furnishes the equation

* For the middle span of the Illinois and St. Louis Bridge we have R=736 feet, and the mean value of $v=6$ feet. The fraction $\frac{v^2}{R^2}$ is therefore $\frac{1}{18\frac{1}{4}\frac{1}{9}}$, and the error committed by omitting this fraction will be less than $\frac{1}{1000}$ of one per cent.

† In practice the variations in the cross-section of the arch will not be very great, and, by assuming the cross-section to be constant, we may arrive at a very close approximation to the real values of the resistances at the abutments without knowing the value of θ. It will, however, be shown hereafter (see page 496) how variations of the cross-sections may be considered.

* Employing the known integrals

$$\int d\,\delta\sin.\delta=-\cos.\delta,$$

$$\int d\,\delta\cos.\delta=\sin.\delta,$$

$$\int d\,\delta\sin.^2\delta=\tfrac{1}{2}\,(\delta-\sin.\delta\cos.\delta).$$

$$11\begin{cases} A-B=-\frac{qR}{4}(\beta+2\beta\sin.^2\beta+3\sin.\beta\cos.\beta)=-\frac{qR}{4}\mu, \\ \text{if } \mu=\beta+2\beta\sin.^2\beta+3\sin.\beta\cos.\beta. \end{cases}$$

Now, since we have for the undeflected arc s and the deflected arc s_1

$$dx=ds\cos.\delta, \qquad dx_1=ds_1\cos.\delta_1,$$
$$dy=ds.\sin.\delta, \qquad dy_1=ds_1\sin.\delta_1,$$

and $ds=ds_1=-R\,d\delta$, there follows very nearly*

$$dx_1-dx=-Rd\delta(\cos.\delta_1-\cos.\delta)=Rd\delta(\delta_1-\delta)\sin.\delta,$$
$$dy_1-dy=-R\,d\delta(\sin.\delta_1-\sin.\delta)=-R\,d\delta(\delta_1-\delta)\cos.\delta.$$

Hence, multiplying the equation (10) by $R\,d\delta\sin.\delta$, or $-R\,d\delta\cos.\delta$, and integrating,† introducing at the same time the auxiliaries,

$$12\begin{cases} G=-P(1-c)\sin.\propto-Q(1-c)\cos.\propto+\frac{qR}{2}\sin.^2\propto+\frac{qR}{8}-\frac{qR}{4}\sin.^2\beta, \\ H=\frac{qR}{4}+\frac{qR}{4}\sin.^2\beta, \end{cases}$$

We find for the different parts of the arch the horizontal and vertical deflections

$$13a/\ x_1-x=\frac{R^3}{\varepsilon\theta}\left\{(G+H)(\sin.\delta-\delta\cos.\delta)-(P-qa)\frac{\sin.^2\delta}{2}+Q\frac{\delta-\sin.\delta\cos.\delta}{2}-\frac{qR}{12}\sin.^2\delta-A\cos.\delta+C\right\}$$

$$13b/\ x_1-x=\frac{R^3}{\varepsilon\theta}\left\{(G-H)(\sin.\delta-\delta\cos.\delta)-(P-qa+qR\sin.\beta)\frac{\sin.^2\delta}{2}+Q\frac{\delta-\sin.\delta\cos.\delta}{2}-B\cos.\delta+D\right\}$$

$$14a/\ y_1-y=\frac{R^3}{\varepsilon\theta}\left\{-(G+H)(\cos.\delta+\delta\sin.\delta)+(P-qa)\frac{\delta+\sin.\delta\cos.\delta}{2}-Q\frac{\sin.^2\delta}{2}-\frac{qR}{12}\cos.^2\delta-A\sin.\delta+E\right\}$$

$$14b/\ y_1-y=\frac{R^3}{\varepsilon\theta}\left\{-(G-H)(\cos.\delta+\delta\sin.\delta)+(P-qa+qR\sin.\beta)\frac{\delta+\sin.\delta\cos.\delta}{2}-Q\frac{\sin.^2\delta}{2}-B\sin.\delta+F\right\}$$

The equations 13a and 13b must be identical for $\delta=\delta$. This gives us the relation

$$C-D=-2H(\sin.\beta-\beta\cos.\beta)-qR\frac{\sin.^2\beta}{2}+qR\frac{\sin.^2\beta}{12}+(A-B)\cos.\beta$$

Substituting the values of H (from 12) and of (A−B) (from 11), we find

$$15\begin{cases} C-D=-qR\left(\sin.\beta+\frac{\sin.^2\beta}{6}\right)=-qR\nu, \text{ if} \\ \nu=\sin.\beta+\frac{\sin.^2\beta}{6} \end{cases}$$

In a similar manner we obtain from the condition that also equations 14a and 14b must be identical for $\delta=\beta$, the relation

$$16\begin{cases} E-F=qR\left\{\frac{\cos.\beta}{2}+\frac{\beta\sin.\beta}{2}-\frac{\cos.^2\beta}{6}\right\}=qR\xi, \text{ if} \\ \xi=\frac{\cos.\beta}{2}+\frac{\beta\sin.\beta}{2}-\frac{\cos.^2\beta}{6} \end{cases}$$

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* $\cos.\delta_1-\cos.\delta=-2\sin.\frac{\delta_1+\delta}{2}\sin.\frac{\delta_1-\delta}{2}$; $\sin.\delta_1-\sin.\delta=2\sin.\frac{\delta_1-\delta}{2}.\cos.\frac{\delta_1+\delta}{2}$.

The deflection of the arch being very slight, the angles δ_1 and δ will be very nearly equal, hence we may assume

$\frac{\delta_1+\delta}{2}=\delta$, and $\sin.\frac{\delta_1-\delta}{2}=\frac{\delta_1-\delta}{2}$ and now

$\cos.\delta_1-\cos.\delta=-(\delta_1-\delta)\sin.\delta$, and $\sin.\delta_1-\sin.\delta=(\delta_1-\delta)\cos.\delta$.

† Employing the known integrals already used, and also

$$\int d\delta.\,\delta\sin.\delta=\sin.\delta-\delta\cos.\delta$$
$$\int d\delta.\sin.\delta\cos.\delta=\tfrac{1}{2}\sin.^2\delta$$
$$\int d\delta.\sin.^2\delta\cos.\delta=\tfrac{1}{3}\sin.^3\delta$$
$$\int d\delta.\,\delta\cos.\delta=\cos.\delta+\delta\sin.\delta$$
$$\int d\delta\cos.^2\delta=\tfrac{1}{2}(\delta+\sin.\delta\cos.\delta)$$
$$\int d\delta\cos.^2\delta\sin.\delta=-\tfrac{1}{3}\cos.^3\delta$$

The equations 13*a* and 13*b* must give $x_1 - x = 0$ for $\delta = +\alpha$ or $\delta = -\alpha$. These values of δ substituted, and the equations subtracted, gives with the values of A−B from 11, and of C−D from 15, the equation

17. $$0 = 2\,G\,(\sin.\alpha - \alpha\cos.\alpha) + q\,R\,\frac{\sin.^2\alpha}{2}\sin.\beta + Q\,(\alpha - \sin.\alpha\cos.\alpha) - q\,R\frac{\sin.^2\alpha}{12} + \frac{qR}{4}\,\mu\cos.\alpha - q\,R\,\nu.$$

The equations 14*a* and 14*b* must give $y_1 - y = 0$ for $\delta = +\alpha$ or $\delta = -\alpha$. By subtracting the equations, after substituting $+\alpha$ or $-\alpha$ for δ, we obtain

18. $$0 = -2\,H\,(\cos.\alpha + \alpha\sin.\alpha) + \left(P - q\,a + q\,R\,\frac{\sin.\beta}{2}\right)(\alpha + \sin.\alpha\cos.\alpha) - q\,R\frac{\cos.^2\alpha}{12} - (A-B)\sin.\alpha + q\,R\xi.$$

Adding equation 13*a*, with $\delta = +\alpha$, and 13*b* with $\delta = -\alpha$, we obtain

19. $$C + D = -2\,H\,(\sin.\alpha - \alpha\cos.\alpha) + \left(P - q\,a + \frac{q\,R\sin.\beta}{2}\right)\sin.^2\alpha + \frac{qR}{12}\sin.^2\alpha + (A+B)\cos.\alpha.$$

Adding equation 14*a* with $\delta = +\alpha$, and 14*b* with $\delta = -\alpha$, we obtain

20. $$E + F = 2\,G\,(\cos.\alpha + \alpha\sin.\alpha) + Q\sin.^2\alpha + \frac{q\,R}{2}\sin.\beta\,(\alpha + \sin.\alpha\cos.\alpha) + \frac{q\,R}{12}\cos.^2\alpha + (A-B)\sin.\alpha.$$

Equations (17) and (18) contain besides P and Q only the unknown quantity (A+B). In order to find a third equation, which would enable us to determine these values, we have to consider the peculiar manner in which the ends of the arch are connected with, or pressing against the abutments. We shall consider the following distinct cases:

I. Both ends of the arch are firmly held to the abutments in such a manner that the tangents to the arch at the ends cannot change their direction.

II. The arch is firmly held at the loaded end and resting with the unloaded end on a pivot.

III. The arch rests with the loaded end on a pivot and is firmly held at the unloaded end.

IV. Both ends of the arch rest on pivots.

If in cases II, III and IV the pivots are $\left\{\begin{matrix}\text{below}\\ \text{in}\\ \text{above}\end{matrix}\right\}$ the centre line of the arch, b or b_1 will be $\left\{\begin{matrix}\text{positive}\\ 0\\ \text{negative}\end{matrix}\right\}$

I. ARCH FIRMLY HELD AT BOTH ENDS.

Equation 10*a* must give $\delta_1 - \delta = 0$ for $\delta = \alpha$, and equation 10*b* must give $\delta_1 - \delta = 0$ for $\delta = -\alpha$. Both equations subtracted give, if we substitute at the same time the value for A−B by 11, and using the auxiliary G by (12)

$$0 = 2\,G\,\alpha + q\,R\cos.\alpha\sin.\beta + 2\,Q\sin.\alpha - g\frac{R}{4}\sin.\alpha\cos.\alpha - \frac{qR}{4}\,\mu \text{ or}$$

Ia. $$G = -Q\,\frac{\sin.\alpha}{\alpha} + q\frac{R}{8}\,\frac{\sin.\alpha\cos.\alpha}{\alpha} - \frac{q\,R}{2}\,\frac{\cos.\alpha}{\alpha}\sin.\beta + \frac{q\,R}{8\alpha}\,\mu$$

Substituting the value of G in equation 17, and introducing the auxiliaries

Ib. $$\begin{cases} K = \alpha + \sin.\alpha\cos.\alpha - \dfrac{2\sin.^2\alpha}{\alpha} \\ K_1 = \dfrac{\sin.^2\alpha}{12} - \dfrac{\sin.\alpha\cos.\alpha}{4\alpha} \\ K_2 = \dfrac{\sin.^2\alpha}{2} - \dfrac{\cos.\alpha}{\alpha}\,(\sin.\alpha - \alpha\cos.\alpha) \end{cases}$$

We obtain for the Q the equation

Ic. $$Q = \frac{qR}{K}\left\{K_1 - K_2\sin.\beta - \frac{\sin.\alpha}{4\alpha}\mu + \nu\right\}$$

(For μ and ν, see equations 11 and 15.)

By adding the equations 10*a* with $\delta = \alpha$, and 10*b* with $\delta = -\alpha$, we obtain

Id. $$0 = 2\,H\alpha - 2\,(P - q\,R\sin.\alpha + \frac{q\,R}{2}\sin.\beta)\cos.\alpha - \frac{q\,R}{4}\sin.\alpha\cos.\alpha + (A+B)$$

And if we multiply this equation by $\sin.\alpha$, and add it to equation 18, substituting at the same time for H its value by 12, and using the auxiliaries,

Ie. $$\begin{cases} K'_1 = \sin.\alpha + \dfrac{2\cos.\alpha + \cos.\alpha\sin.^2\alpha}{6\,(\alpha - \sin.\alpha\cos.)\alpha} \\ K'_2 = \dfrac{\cos.\alpha}{2\,(\alpha - \sin.\alpha\cos.\alpha)} \\ K'_3 = \dfrac{1}{\alpha - \sin.\alpha\cos.\alpha}, \end{cases}$$

we find

$$\text{If. } P = q\,R\left\{K'_1 - \frac{\sin.\beta}{2} + K'_2 \sin.^2\beta - K'_3\,\xi\right\}$$

(for ξ see equation 16).

Let us now substitute for G in I*a* its value by 12, and for C its value $\left(=\frac{b}{R}\right)$ by 8, and we obtain

$$\text{Ig. } b\,(P \sin.\alpha + Q\cos.\alpha) = P\,R \sin.\alpha + Q\,R\left(\cos.\alpha - \frac{\sin.\alpha}{\alpha}\right)$$

$$-\frac{q\,R^2}{8\alpha}(\alpha - \sin.\alpha\cos.\alpha + 4\alpha\sin.^2\alpha) - \frac{q\,R^2}{2\alpha}\cos.\alpha\sin.\beta + \frac{q\,R^2}{4}\sin.^2\beta + \frac{q\,R^2}{8\alpha}\mu$$

After Q and P, the horizontal and vertical resistances of the abutment, have been computed by I*e* and I*f*, we can compute $b\ (P \sin.\alpha + Q\cos.\alpha)$, the moment of flexure produced by the resistance of the abutment, by equation I*g*. The resistances of the abutment at the other end of the arch P_1, Q_1 and b_1 $(P_1 \sin.\alpha + Q_1 \cos.\alpha)$ may then be computed by equations 3, 4 and 5.

For the case of a load extending uniformly over the whole span ($\beta = -\alpha$), the formulæ Ic, I*f* and I*g* may be simplified

$$\text{Ic}'\ Q = \frac{q\,R}{K}\,.\,2K_1$$

$$\text{If}'\ P = q\,R \sin.\alpha$$

$$\text{Ig}'\ b\,(P \sin.\alpha + Q\cos.\alpha) = Q\,R\left(\cos.\alpha - \frac{\sin.\alpha}{\alpha}\right) + q\,R^2\left(-\tfrac{1}{4} + \frac{\sin.^2\alpha}{2} + \frac{\sin.\alpha\cos.\alpha}{4\alpha}\right)$$

To compute the horizontal and vertical deflections of the arch, we have first to determine the constants A B C D E and F. The values of A−B, C−D and E−F, are known by the equations 11, 15 and 16.

(A+B) can be determined by I*d*;
(C+D) and (E+F) by 19 and 20.

The values of (A−B) and (A+B), (C−D) and (C+D), (E−F) and (E+F) being known, the values of A B C D E and F are easily found, and then the horizontal and vertical deflections can be computed by the equations 13 and 14.

II. ARCH FIRMLY HELD AT THE LOADED END, AND THE UNLOADED END RESTING ON A PIVOT.

The centre of the pivot will in this case be the point H of our Fig. 1, and hence b_1 will be a known quantity. Substituting the values of P_1 and Q_1 by 3 and 4 in equation 5, and dividing the equation by R, we obtain

$$\frac{b_1}{R}(-P \sin.\alpha + Q\cos.\alpha + qna \sin.\alpha) = \frac{b}{R}(P \sin.\alpha + Q\cos.\alpha) - 2\,P \sin.\alpha + 2\,qna \sin.\alpha - qn^2a\frac{\sin.\alpha}{2}$$

If we add P sin.α + Q cos.α on each side of the equation sign, and put

$$\tau = 1 - \frac{b}{R} = 1 - C\,; \qquad \tau_1 = 1 - \frac{b_1}{R} = 1 - C_1,$$

Substituting also R sin.α for *a*, and $\frac{\sin.\alpha - \sin.\beta}{\sin.\alpha}$ for *n*, we find

$$\text{II}a/\ \tau\,(P \sin.\alpha + Q\cos.\alpha) = \tau_1(-P \sin.\alpha + Q\cos.\alpha + q\,R \sin.^2\alpha - q\,R \sin.\alpha \sin.\beta) + q\,R\frac{\sin.^2\alpha}{2} - \frac{q\,R \sin.^2\beta}{2}$$

As the arch is firmly held at the loaded end, equation 10*a* must give $\delta_1 - \delta = 0$ for $\delta = \alpha$. Introducing further for $\tau\,(P \sin.\alpha + Q\cos.\alpha)$ in 10*a* the value by equation II*a*, we obtain

$$\text{II}b.\ A = P[\cos.\alpha - \tau_1\alpha\sin.\alpha] + Q[-\sin.\alpha + \tau_1\alpha\cos.\alpha] + q\,R[-\tfrac{3}{4}\sin.\alpha\cos.\alpha - \frac{\alpha}{4} + \tau_1\alpha\sin.^2\alpha] - q\,R\,\tau_1\alpha \sin.\alpha \sin.\beta - \frac{q\,R}{2}\alpha \sin.^2\beta,$$

further

A+B = 2 A−(A−B) or by II*b* and 11

$$\text{II}c.\ A + B = P[2\cos.\alpha - 2\,\tau_1\alpha \sin.\alpha] + Q[-2 \sin.\alpha + 2\,\tau_1\alpha\cos.\alpha] + q\,R\left(-\frac{3}{2}\sin.\alpha\cos.\alpha - \frac{\alpha}{2} + 2\tau_1\alpha\sin.^2\alpha\right) - 2\,q\,R\,\tau_1\alpha \sin.\alpha \sin.\beta - q\,R\alpha \sin.^2\beta + \frac{q\,R}{4}\mu.$$

With this value of A+B, and by putting

$$\text{II}d.\quad \begin{cases} t = \alpha - \sin.\alpha \cos.\alpha + 2\,\tau_1 \alpha \sin.^2\alpha \\ t_1 = 2\sin.^2\alpha - 2\,\tau_1\,\alpha\,\sin.\,\alpha\,\cos.\,\alpha \\ t_2 = \dfrac{\sin.^2\alpha\,\cos.\alpha}{4} - \dfrac{3}{4}\,\alpha\,\sin.\,\alpha - \dfrac{\cos.^2\alpha}{3} - 2\,\tau_1\,\alpha\,\sin.^2\alpha \\ t_3 = \dfrac{\alpha}{2} + \dfrac{\sin.\alpha\,\cos.\alpha}{2} + 2\tau_1\alpha\sin.^2\alpha \\ t_4 = -\dfrac{\cos.\alpha}{2} + \dfrac{\alpha\,\sin.\,\alpha}{2} \end{cases}$$

We obtain from equation 18,

$$\text{II}e.\quad 0 = \text{P}\,t + \text{Q}\,t_1 + q\,\text{R}\left\{ t_2 + t_3\sin.\beta + t_4\sin.^2\beta - \frac{\sin.\alpha}{4}\mu + \xi \right\}$$

If we substitute the value of τ (P sin.α + Q cos.α) by IIa. in the first of equation 12 we obtain

$$\text{II}f.\quad \text{G} = -\tau_1\,(-\text{P}\sin.\alpha + \text{Q}\cos.\alpha + q\,\text{R}\sin.^2\alpha - q\,\text{R}\sin.\alpha\sin.\beta) + \frac{q\,\text{R}}{8} + \frac{q\,\text{R}\sin.^2\beta}{4}$$

and now by 17 and with the auxiliaries

$$\text{II}g.\quad \begin{cases} \delta = 2\,\tau_1\sin.\alpha\,(\sin.\alpha - \alpha\cos.\alpha) \\ \delta_1 = \alpha - \sin.\alpha\cos.\alpha - 2\,\tau_1\cos.\alpha\,(\sin.\alpha - \alpha\cos.\alpha) \\ \delta_2 = -\dfrac{\sin.^3\alpha}{12} + \dfrac{\sin.\alpha - \alpha\cos.\alpha}{4} - 2\,\tau_1\sin.^2\alpha\,(\sin.\alpha - \alpha\cos.\alpha) \\ \delta_3 = \dfrac{\sin.^3\alpha}{2} + 2\,\tau_1\sin.\alpha\,(\sin.\alpha - \alpha\cos.\alpha) \\ \delta_4 = \dfrac{\sin.\alpha - \alpha\cos.\alpha}{2} \end{cases}$$

$$\text{II}h.\quad 0 = \text{P}\,\delta + \text{Q}\,\delta_1 + q\,\text{R}\left\{ \delta_2 + \delta_3\sin.\beta + \delta_4\sin.^2\beta + \frac{\cos.\alpha}{4}\mu - v \right\}$$

Let us now put

$$\text{II}i.\quad \begin{cases} \text{K} = t\,\delta_1 - t_1\,\delta \\ \text{K}_1 = t_1\,\delta_2 - t_2\,\delta_1 \\ \text{K}_2 = t_1\,\delta_3 - t_3\,\delta_1 \\ \text{K}_3 = t_1\,\delta_4 - t_4\,\delta_1 \\ \text{K}_4 = t_1\dfrac{\cos.\alpha}{4} + \dfrac{\sin.\alpha}{4}\,\delta_1 \end{cases} \qquad \begin{cases} \text{K}'_1 = \delta\,t_2 - \delta_2\,t \\ \text{K}'_2 = \delta\,t_3 - \delta_3\,t \\ \text{K}'_3 = \delta\,t_4 - \delta_4\,t \\ \text{K}'_4 = \delta\left(-\dfrac{\sin.\alpha}{4}\right) - \dfrac{\cos.\alpha}{4}\,t \end{cases}$$

And the two equations, IIe. and IIh., will give us

$$\text{II}j.\quad \text{P} = \frac{q\,\text{R}}{\text{K}}\,[\text{K}_1 + \text{K}_2\sin.\beta + \text{K}_3\sin.^2\beta + \text{K}_4\,\mu - t_1\,\nu - \delta_1\,\xi]$$

$$\text{II}k.\quad \text{Q} = \frac{q\,\text{R}}{\text{K}}\,[\text{K}'_1 + \text{K}'_2\sin.\beta + \text{K}'_3\sin.^2\beta + \text{K}'_4\,\mu + t\,\nu + \delta\,\xi]$$

P and Q being found, we obtain P_1 and Q_1 by 3 and 4 and then b or the moment b (P sin.α + Q cos.α) by 5.

The constants A B C D E and F required for the computation of the deflection will be obtained from the values of (A−B), (C−D) and (E−F) by 11, 15 and 16, and those of (A+B), (C+D) and (E+F) by (IIc) 19 and 20.

III. ARCH FIRMLY HELD AT UNLOADED END, AND THE LOADED END RESTING ON A PIVOT.

G Fig. 1 will be the centre of the pivot, hence b is a known quantity.

The arch being firmly held at the unloaded end, equation 10b must give $\delta_1 - \delta = 0$ for $\delta = -\alpha$, and putting $\tau = 1 - c$ we obtain

$$\text{III}a.\quad \text{B} = \text{P}\,(\cos.\alpha - \tau\,\alpha\,\sin.\,\alpha) + \text{Q}\,(\sin.\alpha - \tau\,\alpha\,\cos.\alpha) + q\,\text{R}\left(\frac{\alpha\sin.^2\alpha}{2} - \sin.\alpha\cos.\alpha\right) + q\,\text{R}\,\sin.\beta\cos.\alpha - \frac{q\,\text{R}}{2}\,\alpha\sin.^2\beta$$

We find further from A+B=(A−B) + 2 B = 2 B$-\frac{q\,\text{R}}{4}\,\mu$

$$\text{III}b.\quad \text{A} + \text{B} = 2\,\text{P}\,(\cos.\alpha - \tau\,\alpha\sin.\alpha) + 2\,\text{Q}\sin.\alpha - \tau\,\alpha\cos.\alpha) + q\,\text{R}\,(\sin.^2\alpha - 2\sin.\alpha\cos.\alpha) + 2\,q\,\text{R}\sin.\beta\cos.\alpha - q\,\text{R}\,\alpha\sin.^2\beta - \frac{q\,\text{R}}{4}\,\mu.$$

Substituting this value in 18, and putting

$$\text{III}c.\quad \begin{cases} t = \alpha - \sin.\alpha\cos.\alpha + 2\,\tau\,\alpha\sin.^2\alpha \\ t_1 = -2\sin.^2\alpha + 2\,\tau\,\alpha\sin.\alpha\cos.\alpha \\ t_2 = -\dfrac{\cos.\alpha}{4} - \dfrac{5}{4}\,\alpha\sin\alpha - \dfrac{\cos.^2\alpha}{12} + \sin.^2\alpha\cos.\alpha - \alpha\sin.^2\alpha \\ t_3 = \dfrac{\alpha}{2} - \dfrac{3}{2}\sin.\alpha\cos.\alpha \\ t_4 = -\dfrac{\cos.\alpha}{2} + \dfrac{\alpha\sin.\alpha}{2} \end{cases}$$

we obtain

III*d*. $0 = P\,t + Q\,t_1 + q\,R\,(t_2 + t_3 \sin.\beta + t_4 \sin.^2\beta + \frac{\sin.\alpha}{4}\mu + \xi)$

If we substitute in 17 the value of G by 12, and put

III*e*.
$$\begin{cases} \delta = -2\,\tau \sin.\alpha\,(\sin.\alpha(-\alpha\cos.\alpha) \\ \delta_1 = -2\,\tau \cos.\alpha\,(\sin.\alpha(-\alpha\cos.\alpha) + \alpha(-\sin.\alpha\cos.\alpha) \\ \delta_2 = (\tfrac{1}{4} + \sin.^2\alpha)(\sin.\alpha(-\alpha\cos.\alpha) - \frac{\sin.^3\alpha}{12} \\ \delta_3 = \frac{\sin.^3\alpha}{2} \\ \delta_4 = -\tfrac{1}{2}\sin.\alpha(-\alpha\cos.\alpha) \end{cases}$$

we obtain

III*f*. $0 = P\,\delta + Q\,\delta_1 + q\,R \left\{ \delta_2 + \delta_3 \sin.\beta + \delta_4 \sin.^2\beta + \frac{\cos.\alpha}{4}\mu - \nu \right\}$

Putting now

III*g*.
$$\begin{cases} K = t\,\delta_1 - t_1\,\delta \\ K_1 = t_1\,\delta_2 - t_2\,\delta_1 \\ K_2 = t_1\,\delta_3 - t_3\,\delta_1 \\ K_3 = t_1\,\delta_4 - t_4\,\delta_1 \\ K_4 = \frac{\cos.\alpha}{4}t_1 - \frac{\sin.\alpha}{4}\delta_1 \end{cases} \qquad \begin{cases} K'_1 = \delta\,t_2 - \delta_2\,t \\ K'_2 = \delta\,t_3 - \delta_3\,t \\ K'_3 + \delta\,t_4 - \delta_4\,t \\ K'_4 = \delta\frac{\sin.\alpha}{4} - \frac{\cos.\alpha}{4}t, \end{cases}$$

we find by III*d* and III*f*.

III*h*. $P = \frac{q\,R}{K}\,[K_1 + K_2 \sin.\beta + K_3 \sin.^2\beta + K_4\,\mu - t_1\,v + \delta_1\,\xi]$

III*i*. $Q = \frac{q\,R}{K}\,[K'_1 + K'_2 \sin.\beta + K'_3 \sin.^2\beta + K'_4\,\mu + t\,v + \delta\,\xi]$

P and Q once known, we obtain P_1, Q_1, and b_1 ($P_1 \sin.\alpha + Q_1 \cos.\alpha$) by 3, 4 and 5.

The constants A, B, C, D, E and F are obtained by 11, 15, 16, III*b*., 19 and 20.

IV. BOTH ENDS OF THE ARCH PIVOTED, OR *b* AND b_1 KNOWN.

If we substitute in 5 for P_1 Q_1 and $n\,a$ their values by 3, 4 and 1, we obtain

$P \sin.\alpha(-b_1 - b + 2R) = Q \cos.\alpha(b - b_1) + q\,R \sin.^2\alpha(-b_1 + \tfrac{3}{2}R) + q\,R \sin.\beta \sin\alpha(b_1 - R) - \frac{q\,R^2 \sin.^2\beta}{2}$;

and putting

IV*a*. $\tau = 1 - c = 1 - \frac{b}{R}$; $\tau_1 = 1 - c_1 = 1 - \frac{b_1}{R}$;

we find

IV*b*. $P \sin.\alpha = Q \cos.\alpha \frac{\tau_1 - \tau}{\tau + \tau_1} + q\,R \sin.^2\alpha \frac{1 + 2\,\tau_1}{2\,(\tau + \tau_1)} - q\,R \sin.\beta \sin.\alpha \frac{\tau_1}{\tau + \tau_1} - \frac{q\,R \sin.^2\beta}{2\,(\tau + \tau_1)}$

Equation 12 becomes with this value of P sin. α,

IV*c*. $G = -Q \cos.\alpha \frac{2\,\tau\,\tau_1}{\tau + \tau_1} + \frac{q\,R}{8} + \frac{q\,R \sin.^2\alpha}{2} \cdot \frac{\tau_1 - 2\tau\,\tau_1}{\tau + \tau_1} + q R \sin.\alpha \sin.\beta \frac{\tau\,\tau_1}{\tau + \tau_1} + \frac{q\,R \sin.^2\beta}{4} \frac{\tau - \tau_1}{\tau + \tau_1}$,

and now by 17 and with the auxiliaries,

IV*d*. $\tau_2 = \frac{\tau}{\tau + \tau_1}$; $\tau_3 = \frac{\tau_1}{\tau + \tau_1}$; $\tau_4 = \frac{\tau\,\tau_1}{\tau + \tau_1}$

IV*e*.
$$\begin{cases} K = -4\,\tau_4 \cos.\alpha\,(\sin.\alpha(-\alpha\cos.\alpha) + \alpha(-\sin.\alpha\cos.\alpha) \\ K_1 = -(\sin.\alpha(-\alpha\cos.\alpha)\,[\tfrac{1}{4} + \sin.^2\alpha(\tau_3 - 2\,\tau_4)\,] + \frac{\sin.^3\alpha}{12} \\ K_2 = -\frac{\sin.^3\alpha}{2} - 2\sin.\alpha\,(\sin.\alpha(-\alpha\cos.\alpha)\,\tau_4 \\ K_3 = \tfrac{1}{2}\,(\sin.\alpha(-\alpha\cos.\alpha)\,(\tau_3 - \tau_2) \end{cases}$$

IV*f*. $Q = \frac{q\,R}{K}\left\{ K_1 + K_2 \sin.\beta + K_3 \sin.^2\beta - \frac{\cos.\alpha}{4}\mu + \nu \right\}$

P will be obtained by substituting the value of Q by IV*f* in IV*b*.

The constants A, B, C, D, E and F for the computation of the deflections can be computed from the values of A−B, C−D, E−F', by 11, 15 and 16, and of A+B by 18, C+D by 19, and E + F by 20.

In the particular case (IV') when both pivots are in the centre-line of the arch, or $b = b_1 = 0$, the auxiliaries become

IV'*a*. $\tau' = \tau'_1 = 1$

IV'*d*. $\tau'_2 = \tau'_3 = \tau'_4 = \tfrac{1}{2}$

IV′e. $\begin{cases} K'=3\ (\alpha-\sin.\alpha\cos.\alpha)-2\alpha\sin.^2\alpha \\ K'_1=-\frac{1}{4}\ (\sin.\alpha\cos.\alpha)\ (1-2\sin.^2\alpha)+\frac{\sin.^2\alpha}{12} \\ K'_2=-\frac{1}{4}\sin.^2\alpha+\alpha\sin.\alpha\cos.\alpha \\ K'_3=0 \end{cases}$

We can further in this case compute P directly by 5, which becomes with $b=b_1=0$.

$$\text{IV}'g.\quad P=q\,n\,a\left(1-\frac{n}{4}\right)$$

EFFECT OF CHANGES OF TEMPERATURE UPON THE ARCH.

If λ is the coefficient of expansion for a given change of temperature, the resistances of the abutments created by that change may be considered as the forces which, applied to the freely expanded arch of the span $2a\ (1+\lambda)$, would force it back to the original span $2a$. The coefficient λ being a very small fraction, we may also, very approximately, regard the resistances spoken of, as the forces which would reduce the span $2a$ of the arch for the length $2\lambda a$. As in the investigation of the effect of load, we may assume these forces to consist of vertical and horizontal forces P, Q, and P_1, Q_1, acting on points G and H in the normal end-planes of the arch (Fig. 1).

The conditions for the equilibrium of the arch are

21. $P_1=-P$

22. $Q_1=Q$

23. $b_1\ (P_1\sin.\alpha+Q_1\cos.\alpha)=b\ (P\sin.\alpha+Q\cos.\alpha)-2Pa.$

The moment of flexure for any point F will be

$$M=-P\,(x-b\sin.\alpha)+Q\,(y+b\cos.\alpha)$$

or by $x=R\,(\sin.\alpha-\sin.\delta)$; $y=R\,(\cos.\delta-\cos.\alpha)$; and $1-\frac{b}{R}=\tau$;

24. $M=-PR\,(\tau\sin.\alpha-\sin.\delta)+QR\,(\cos.\delta-\tau\cos.\alpha).$

Substituting this value of M in equation 7, and integrating, we obtain

25. $\delta_1-\delta=\frac{R^2}{2\theta}\,[-\tau\,(P\sin.\alpha+Q\cos.\alpha)\,\delta-P\cos.\delta+Q\sin.\delta+A]$

Multiplying 25 by $R\,d\delta\sin.\delta$ or $-R\,d\delta\cos.\delta$, and integrating, we reduce as on page 486,

26. $x_1-x=\frac{R^2}{\xi\theta}\left\{-\tau(P\sin.\alpha+Q\cos.\alpha)\,(\sin.\delta-\delta\cos.\delta)-P\frac{\sin.^2\delta}{2}+Q\frac{\delta-\sin.\delta\cos.\delta}{2}-A\cos.\delta+C\right\}$

27. $y_1-y=\frac{R^2}{\xi\theta}\left\{\tau\,(P\sin.\alpha+Q\cos.\alpha)\,(\cos.\delta+\delta\sin.\delta)+P\frac{\delta+\sin.\delta\cos.\delta}{2}-Q\frac{\sin.^2\delta}{2}-A\sin.\delta+E\right\}$

Assuming now that the left end of the arch did not change its position, or that equation 26 gives $x_1-x=0$ for $\delta=\alpha$, the same equation with $\delta=-\alpha$ must give $x_1-x=-2\lambda a$.

The first of these conditions gives us

28. $C=\tau\,(P\sin.\alpha+Q\cos.\alpha)\,(\sin.\alpha-\alpha\cos.\alpha)+P\frac{\sin.^2\alpha}{2}-Q\frac{\alpha\sin.\alpha\cos.\alpha}{2}+A\cos.\alpha;$

and by the second we obtain, with the value of C by 28,

29. $-2\lambda a=\frac{R^2}{\xi\theta}\,[2\tau\,(P\sin.\alpha+Q\cos.\alpha)\,(\sin.\alpha-\alpha\cos.\alpha)-Q\,(\alpha-\sin.\alpha\cos.\alpha)]$

Equation 27 must give $y_1-y=0$ for $\delta=\alpha$, hence

30. $E=-\tau\,(P\sin.\alpha+Q\cos.\alpha)\,(\cos.\alpha+\alpha\sin.\alpha)=P\frac{\alpha+\sin.\alpha\cos.\alpha}{2}+Q\frac{\sin.^2\alpha}{2}+A\sin.\alpha.$

Equation 27 must also give $y_1-y=0$ for $\delta=-\alpha$, hence with the value of E by 30,

31. $0=-P(\alpha+\sin.\alpha\cos.\alpha)+2A\sin.\alpha$

We shall now apply these equations to the different cases I, II, III and IV, considered in investigating the effect of load.

I. ARCH FIRMLY HELD AT BOTH ENDS.

Since equation (25) must give $\delta_1-\delta=0$ for $\delta=\alpha$ and for $\delta=-\alpha$, we deduce, by adding the two equations obtained by putting in 25, first $\delta=\alpha$, and then $\delta=-\alpha$,

$$Ih.\quad A=P\cos.\alpha;$$

and by subtracting one from the other,

$$Ii.\ \tau\ (P \sin.\alpha + Q \cos.\alpha) = \frac{Q \sin.\alpha}{\alpha}.$$

Substituting for $\tau(P \sin.\alpha + Q \cos.\alpha)$ in 29 its value by I*i*, and putting, as on page 487,

$$K = \alpha + \sin.\alpha \cos.\alpha - \frac{2 \sin.^2\alpha}{\alpha},$$

we obtain

$$Ij.\quad Q = \frac{2 \lambda \xi \theta \sin.\alpha}{K R^2},$$

By combining equations 31 and I*h*., we find

$$Ik.\quad P = 0;$$

and

$$Il.\quad A = 0.$$

The moment of flexure at the abutment will be obtained by I*i*.

II. ARCH FIRMLY HELD AT THE LOADED END, AND THE UNLOADED END RESTING ON A PIVOT.

Substituting in equation 23 the values of P_1 and Q_1 by 21 and 22, and putting, as before,

$$\tau = 1 - \frac{b}{R} \text{ and } \tau_1 = 1 - \frac{b_1}{R},$$

we deduce

$$IIl.\ \tau\ (P \sin.\alpha + Q \cos.\alpha) = \tau_1\ (-P \sin.\alpha + Q \cos.\alpha)$$

and now as equation 25 must give $\delta_1 - \delta = 0$ for $\delta = \alpha$,

$$IIm.\ A = \tau_1 \alpha\ (-P \sin.\alpha + Q \cos.\alpha) + P \cos.\alpha - Q \sin.\alpha.$$

With this value of A equation 31 becomes

$$IIn.\quad 0 = P t + Q t_1,$$

wherein t and t_1 are the same auxiliaries as employed on page 489 (see equation II*d*).

From equation 29 we deduce by II*l*,

$$IIo.\quad 0 = P \delta + Q \delta_1 - \frac{2 \lambda a \xi \theta}{R^2},$$

δ and δ_1 being the same as by II*g* on page 489.

Putting now $K = t \delta_1 - t_1 \delta$ as by II*i*., we find from II*n*. and II*o*.,

$$IIp.\quad P = -\frac{2 \lambda \xi \theta \sin.\alpha}{R^2} \cdot \frac{t_1}{K}$$

$$IIg.\quad Q = \frac{2 \lambda \xi \theta \sin.\alpha}{R^2} \cdot \frac{t}{K}$$

The value of b, or the moment at the abutment, can be found by II*e*.

III. ARCH FIRMLY HELD AT UNLOADED END, AND THE LOADED END RESTING ON A PIVOT.

Since 25 must give $\delta_1 - \delta = 0$ for $\delta = -\alpha$, we have

$$IIIj.\quad A = -\tau\ (P \sin.\alpha + Q \cos.\alpha)\alpha + P \cos.\alpha + Q \sin.\alpha$$

Equation 31 becomes with this value of A

$$IIIk.\quad 0 + P t + Q t_1$$

wherein t and t_1 have the same values as in III*c*., page 489.

With the auxiliaries δ and δ_1 (see III*e*, page 490) equation 29 may be written

$$IIIl.\quad 0 = P \delta + Q \delta_1 - \frac{2 \lambda a \xi \theta}{R^2}$$

And now with $K = t \delta_1 - t_1 \delta$ as by III*g*, (page 490), we find by III*k* and III*l*,

$$IIIm.\quad H = -\frac{1 \lambda \xi \theta \sin.\alpha}{R^2} \cdot \frac{t_1}{K}$$

$$IIIn.\quad Q = \frac{2 \lambda \xi \theta \sin.\alpha}{R^2} \cdot \frac{t}{K}$$

IV. BOTH ENDS OF THE ARCH PIVOTED, OR b AND b_1 KNOWN.

Substituting for P_1 and Q_1 in 23, their values by 21 and 22, and employing the auxiliaries τ and τ_1 (IV*a*, page 490) we find

$$IVg.\quad P \sin.\alpha = Q \cos.\alpha \frac{\tau_1 - \tau}{\tau + \tau_1}$$

and now by 29, and employing the auxiliary K (see IV*e*, page 490),

$$IVh.\quad Q = \frac{2 \lambda \xi \theta \sin.\alpha}{K' R^2}$$

Q being known, we obtain P by IV*g*, and the constants A, C and E by 31, 28, and 20.

In the particular case IV′, when $b = b_1 = 0$, we find

$$Q = \frac{2 \lambda \xi \theta \sin.\alpha}{K' R^2}$$

(for K′ see IV′*e*, page 491);

and $P = 0$.

These formulæ consider only the effect of a certain *change* of temperature, hence, to compute the resistances of the abutments for a given *degree* of temperature, we must know what they are for some other degree. For this purpose we assume the arch to be made of such size

that at a medium temperature, say 60°F., it fits exactly between the abutments; in which case the temperature will create no strains at all. Assuming further the extreme of temperature to be 80° F.* above and below this medium temperature, we obtain the extreme values of the resistances of the abutments due to temperature, by employing the following values of the coefficient λ (for wrought iron and steel), viz:

for the greatest heat (140° F.)
$$\lambda = 0.000527$$
for the greatest cold (−20° F.)
$$\lambda = -0{,}000527$$

APPLICATION OF THE FORMULÆ.

The strains in an arch bridge, if we omit those caused by wind, arise out of three different causes:

1st. The permanent load.
2d. The movable load.
3d. The temperature.

The permanent load may be considered as being uniformly distributed over the whole length of the span, and we find the resistances of the abutments due to the permanent load, viz:

P' and P'_1 = the vertical components,
Q' and Q'_1 = the horizontal components,

$$\left.\begin{aligned} M'_0 &= b'\,(P' \sin.\,\alpha + Q' \cos.\,\alpha) \\ \text{and } M'_1 &= b'_1\,(P'_1 \sin.\,\alpha + Q'_1 \cos.\,\alpha) \end{aligned}\right\}$$

the moments of flexure at the abutments, by putting in the formulæ deduced for movable load $\beta = -\alpha$, and substituting for q the quantity $q' =$ the permanent load per unit of horizontal length of the arch.

The resistances of the abutments due to the movable load we denote by P'' and P''_1, Q'' and Q''_1, M''_0 and M''_1, and those due to temperature by P''' and P'''_1, Q''' and Q'''_1, M'''_0 and M'''_1.

The total resistances of the abutments caused by permanent load, movable load, and temperature combined will then be:

$$P = P' + P'' + P''' \qquad P_1 = P'_1 + P''_1 + P'''_1$$
$$Q = Q' + Q'' + Q''' \qquad Q_1 = Q'_1 + Q''_1 + Q'''_1$$
$$M_0 = M'_0 + M''_0 + M'''_0 \qquad M_1 = M'_1 + M''_1 + M'''_1$$

If the arch is firmly bolted to the abutments, as in the Illinois and St. Louis Bridge (Fig. 4), the formulæ of

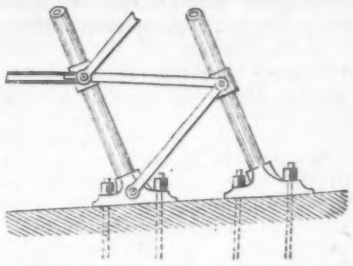


Fig. 4.

case I have to be used; and if the arch rests with each end on a pivot in its center-line (Fig. 5), those of case IV

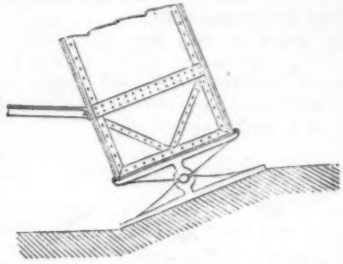


Fig. 5.

will answer for all positions of load and for all degrees of temperature. If, however, each end of the arch is finished with a flat surface (Fig. 6) or with two

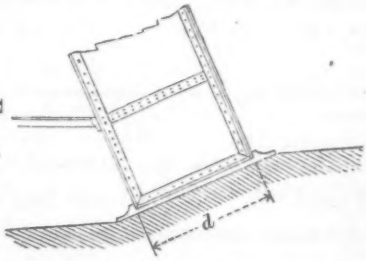


Fig. 6.

pivots (Fig. 7) the bending moment at

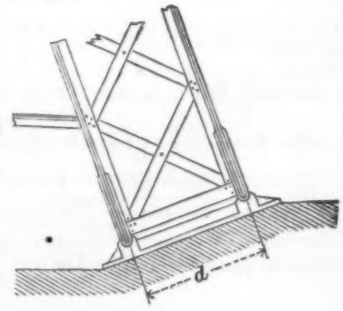
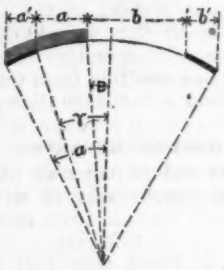


Fig. 7.

* This may appear too much, but we have to make some allowance for inaccuracy of workmanship and also on account of the effect of the compression by the strains in the direction of the arch, as will be shown on page 495.

an abutment may become sufficiently great to lift one edge of the end-face, or one pivot, entirely off the abutment, throwing all the strain on the other edge or pivot. The direction of the tangent to the center-line of the arch, which remained the same as long as the whole end-face or both pivots were in contact with the abutments, is now suddenly changed, and is subject to be changed again by the slightest variation of load or temperature. As this can take place at one abutment only, or at both together, every one of the cases, I, II, III, and IV, may be applicable to the same arch, solely depending upon certain conditions of load and temperature.

In order to find for any particular load or temperature, which case is to be applied, we first compute the total resistances of the abutments by the formulæ of case I, and then compute



$$33 \quad b = \frac{M_o}{P \sin. \alpha + Q \cos. \alpha}$$

$$\text{and } b_1 = \frac{M_1}{P_1 \sin. \alpha + Q_1 \cos. \alpha}$$

Case I will be applicable as long as *both* b and b_1 are between the limits $+\frac{d}{2}$ and $-\frac{d}{2}$ (see Figs. 6 and 7, Page 29).

If *one* of the two quantities $\left\{ \begin{matrix} b_1 \\ b \end{matrix} \right\}$ is beyond these limits (greater than $+\frac{d}{2}$ or smaller than $-\frac{d}{2}$) we have to compute the resistances of the abutments over again by the formulæ of case $\left\{ \begin{matrix} \text{II} \\ \text{III} \end{matrix} \right\}$ putting $\left\{ \begin{matrix} b_1 \\ b \end{matrix} \right\} = +\frac{d}{2}$ or $-\frac{d}{2}$, the sign of $\frac{d}{2}$ to agree with that of $\left\{ \begin{matrix} b_1 \\ b \end{matrix} \right\}$ computed by 33. With the new values of the resistances of the abutments we compute again $\left\{ \begin{matrix} b \\ b_1 \end{matrix} \right\}$ by 33. If we find $\left\{ \begin{matrix} b \\ b_1 \end{matrix} \right\}$ still within the limits $+\frac{d}{2}$ and $-\frac{d}{2}$, the formulæ of case $\left\{ \begin{matrix} \text{II} \\ \text{III} \end{matrix} \right\}$ are applicable; and if we find it beyond these limits the formulæ of case IV have to be used.

If *both* b and b_1 computed by 33 with the resistances of the abutments obtained by the formulæ of case I, are beyond the limits $+\frac{d}{2}$ and $-\frac{d}{2}$, we have also to try first case II or III, assuming that end of the arch as pivoted for which we had obtained the greater absolute value of b or b_1, and we are only justified in using the formulæ of case IV if the new resistances of the abutments as computed by the formulæ of case II or III, substituted in 33, give for the fixed end of the arch still a value absolutely greater than $\frac{d}{2}$.

INTERIOR FORCES OR STRAINS.

On page 483 we separated the total interior force acting in a normal section of the arch into a moment M, and two forces S′ and S″, the former lying in the direction of a tangent to the central curve of the arch, the latter in the direction of the normal. We distinguish S′ as the *strain in the direction of the arch*, S″ as the *shearing strain*.

The quantities M, S′ and S″ due to permanent load, movable load, and temperature combined, for any point (x, y) of the arch can be computed by the following equations:

a/ for loaded part (from $x=0$ to $x=na$)

$$34a \quad M = M_o - Px + Qy + \frac{q'x^2}{2} + \frac{qx^2}{2}$$

$$35a \quad S' = Q \cos. \delta + (P - q'x - qx) \sin. \delta$$

$$36a \quad S'' = Q \sin. \delta - (P - q'x - qx) \cos. \delta$$

b/ for unloaded part (from $x = n\,a$ to $x=2a$)

$$34b \quad M = M_o - P\,x + Q\,y + \frac{q'x^2}{2} + q\,n\,a\left(x - \frac{na}{2}\right)$$

35*b* $S'=Q\cos.\delta+(P-q'x-qna)\sin.\delta$

36*b* $S'=Q\sin.\delta-(P-q'x-qna)\cos.\delta$

As engineers have to deal with moments of flexure and shearing strains in the calculation for every girder or truss I may assume their effect on the different parts of construction (chord and web members) as known, and I will only mention that a positive moment M will create positive or compressive strain in all parts below, and negative or tensile strain in all parts above the neutral axis of the arch.

The strain in the direction of the arch, S′, is always positive and, passing through the center of gravity of the cross-section, will create a positive or compressive strain uniformly distributed over the whole sectional area of the arch. This strain has to be added to those caused by the moment M.

EFFECT OF COMPRESSION BY THE STRAINS IN THE DIRECTION OF THE ARCH.

The effect of these strains has been omitted in the calculation of the resistances of the abutments. We shall now investigate the error produced by this omission.

By the strains in the direction of the arch the elements of the arch are compressed in their original direction, and hence the arch is shortened without being deflected. The distance between the abutments, however, remaining unchanged, the effect of this compression is the same as that of such a decrease of temperature as would cause the same shrinkage of the arch.

Putting A=the sectional area of the arch, the compression per unit of cross-section produced by the strain in the direction of the arch is $\frac{S'}{A}$ and the shortening of the element ds of the arch will be $\frac{S'}{A}\cdot\frac{1}{\varepsilon}\cdot ds$.

As for a given load and temperature no great variations will take place in the strains S′, we may, with sufficient accuracy, assume an average value of S′ and consider it constant throughout the whole arch. We may then compute the resistances of the abutments due to the strains in the direction of the arch, or the amount of the error produced by omitting these strains, by the formulæ deduced for the effect of temperature, using the coefficient

$$\lambda=-\frac{S'}{A\cdot\varepsilon}.$$

If we had always the same condition of load and temperature we could avoid this error entirely, by constructing the arch so much larger than the space between the abutments that the strains in the direction of the arch would compress it to the right size. For our various cases of load and temperature we can at least reduce this error materially by constructing the arch of such size that the strains in the direction of the arch due to the permanent load, and the movable load on one-half of the span, at the medium temperature, will bring it to the right length.* We shall thus have considered an average value of the strains in the direction of the arch, and the errors produced by assuming this average value instead of the true one will be so small that an addition of a few degrees of temperature in computing the strains resulting from this cause, will be found a sufficient allowance for them.

GENERAL REMARKS.

In the preceding pages we have considered only such cases in which the movable load extends from one end of the span, or $x=0$, to a certain point $x=na$. It is, however, clear that our formulæ can also be used to obtain the resistances of the abutments when the movable load does not commence at $x=0$, but extends only from some point $x=n'a$ to some other point $x=na$. We have simply to deduct, in such a case, from the values obtained by our formulæ for a load extending from $x=0$ to $x=na$, those obtained by the same formulæ for a load extending from $x=0$ to $x=n'a$.

For all practical purposes, however, when it is only important to know the maxima of strain which may occur in

* The great accuracy in the construction of the abutments, with regard to their distance and their angle of inclination, which is required for the correctness of the strains, might seem difficult to attain, and even, if attained, may be disturbed again by a slight settling of the abutments. The consequences of such an event, however, would not be serious. Whenever, on account of a slight deviation from the dimensions assumed in the calculation some part of the arch is strained beyond the elastic limit, the permanent set thus caused must necessarily be a step towards restoring the conditions upon which the calculation has been based; hence the arch has a tendency to accommodate itself in course of time to its abutments.

any part of the arch, it will be found sufficient to make the computation for movable loads covering, say, $\frac{1}{3}$, $\frac{1}{2}$, $\frac{2}{3}$..... $\frac{1}{4}$ and the whole span, or for $n=\frac{1}{4}$, $\frac{1}{3}$, $\frac{1}{2}$, ...$\frac{1}{4}$ and $n=2$, and for both extremes of temperature.

The strains due to changes of temperature will be found to be by no means insignificant, and it may be worth while to examine them a little closer. To simplify the case, let us suppose the arch to consist of two members or chords, each of the cross-section A, connected by a suitable system of braces. If δ is the distance between the centers of gravity of the sections of the chords, the moment of inertia of the cross-section of the arch will be very approximately

$$\theta=\frac{A\delta^2}{2}$$

The strain by temperature in one chord may then be expressed by

$$T=\frac{S'}{2}+\frac{M}{8}=\lambda\,\varepsilon\,\theta\left(f+\frac{1}{\delta}.f'\right)=\frac{1}{2}\,\lambda\varepsilon A\,(\delta^2 f+\delta f')$$

wherein f and f' are certain functions of R, $\propto$, x and y. This shows plainly that the strain from temperature is in direct proportion to the sectional area of the chords, or that for a given span, rise and depth of arch the strain per square inch of section due to temperature is also given. We can see now, at once, the advantage of using steel for arch-bridges instead of wrought iron. Assuming, for instance, the greatest strain from temperature to be 4000 pounds per square inch,* and allowing, as a maximum, 10,000 pounds per square inch on wrought iron and 20,000 pounds per square inch on steel, a wrought-iron arch will lose 40% of its total strength by the strains from temperature, while a steel arch of the same general proportions will lose but 20%; or six pounds of steel will reach as far in supporting a given load as sixteen pounds of wrought iron. In large spans, where the weight of the structure forms a great portion of the total load to be carried, the use of steel will be still more advantageous.

The strains from temperature, or the loss of available strength of the material from this cause, can also be diminished by diminishing the depth δ of the arch. The strength of the arch to resist the strain in its own direction is not altered by changing the value of δ, and it is only its ability to resist the moments of flexure due to unequal loads which is impaired thereby, and to restore which the section A of the chords would have to be increased. The most advantageous value of δ will be best found by experiment, but we can see that the greater the spans, and hence the greater the permanent load compared with the movable load, the smaller may be the depth of the arch compared with the span and the smaller will be the loss of strength by the strains from temperature.

In all our calculation, so far, we have assumed the same cross-section of the arch for the whole span. Economy, however, will in most cases demand various cross-sections for different parts of the arch. For instance, in an arch with fixed ends, as those of the St. Louis Bridge, the chords near the abutments are subject to much greater strains than in other portions of the arch, and should therefore be made stronger. In the calculation for the St. Louis Bridge this strengthening of the ends of the arch has been considered in the following manner. Instead of distinguishing, as we have done, only two different parts of the arch, a *loaded* part and an *unloaded* part, there were four different parts, viz.:

a' a loaded end-piece,
a a loaded middle-piece,
b an unloaded middle-piece,
b' an unloaded end-piece.

Instead of having two equations each of 10, 13 and 14 there were four of each kind, and consequently also 12 constants of integration instead of our six. The six additional equations, which are required for the elimination or determination of these constants, are obtained by the following conditions:

$10a'$ and $10a$ } must be identical for $\delta=j$
$13a'$ and $13a$ }
$14a'$ and $14a$ }
$10b$ and $10b'$ } must be identical for $\delta=-j$
$13b$ and $13b'$ }
$14b$ and $14b'$ }

In conclusion, I may remark that all formulæ deduced in the course of this paper may also be applied to a suspended arch by simply changing the sign of q and q', whereby the compressive strains due to load will be changed in tension and *vice versa*.

* We assume the same for steel and wrought iron, as as also λ and ε are about the same for both materials.

SLIDING FRICTION ON AN INCLINED PLANE.

By Prof. A. S. KIMBALL.

From "The American Journal of Science."

The following investigation was undertaken with a desire to demonstrate, if possible, by a laboratory experiment, that the law which affirms that the coefficient of sliding friction is constant for all velocities is not strictly true.

Our results seem to establish the point, at least in the case of bodies sliding down an inclined plane. I am aware that the truth of this law has been questioned; indeed the opinion of very many practical mechanics is directly opposed to it. Long ago Prof. Playfair remarked, as the result of some observations made at the slide of Alpnach, that it would appear that friction is neither proportioned to the pressure nor independent of the velocity. Later observations made at the launching of the Raritan and the Princeton (Jour. Frank. Inst., 3d, VII., 108) showed that the coefficient of friction just before the vessel left the ways was much less than during the first five seconds of its motion. More recent still are the experiments of M. Bochet (Comptes Rendus, April 26, 1858), upon the friction of railway carriages and brakes, which point to the same conclusion; indeed the author goes so far as to give the form of the function which expresses the variation of the coefficient of friction with the velocity, and gives approximate values to its constants for the case of railway trains. His formula is copied by Weisbach with a caution.

Opposed to these views are the careful experiments of Coulomb and Morin, upon which the statements of our textbooks are founded.

The apparatus used in our experiments was simple, but it seems capable of giving very sharp and reliable results. A smooth pine plank $10' \times 12'' \times 2''$ was firmly placed at a measured angle with the horizon and supported throughout by stout beams. Upon this plank was a weight box with pine runners, having a bearing surface of 24 square inches. The cover of the box was about six feet in length, and upon it were placed slips of smoked glass. Firmly fixed above the glass, to an independent support, was a verified tuning fork of 435 complete vibrations per second, carrying a style which lightly touched the glass surface beneath it. The weight box was supported in position at the upper end of the inclined plane by a cord fastened to a screw which served to give the box a very slow upward motion. At the proper time the screw was turned, the fork vibrated, the cord cut or burned off, and the box allowed to slide to the bottom of the plane. The style of the fork at the same time would trace upon the smoked glass a waved line, which would be a perfect autographic register of the experiment. The time of sliding, the velocity at any point, the distance passed over in any unit of time, could all be measured or counted directly from the smoked glass.

The graphical method of working up the experiment was employed as follows: The bottom of a sheet of section paper was made a "time line" ($\frac{1}{435}$ of a sec. = a unit). At various points on this line the corresponding velocities were erected as ordinates. The equation of a line connecting the upper extremities of these ordinates would express the law of the motion studied.

It is evident that this line would have been straight if the acceleration of the slide had been uniform, like that of a body falling in vacuo. If, however, a variable resistance be opposed to the motion of the slide, the acceleration will no longer be uniform, and the line will become curved, concave toward the axis of abscissas, if the resistance is increasing, convex if the resistance diminishes. The acceleration of such a motion at any time will be proportional to the tangent of the angle which the direction of the curve at that point makes with the time line. It is also evident that such acceleration may at once be measured from the paper, since it is the difference between the velocities for two successive units of time. The curve constructed as above, from every experi-

ment made, was decidedly convex toward the time line, showing a constantly decreasing resistance to the motion of the slide as the velocity increased. If we assume that this increase in acceleration was due to a diminished coefficient of friction, the value of the coefficient for any time may be found in the following manner :

Let a, b and h=the altitude, base, and length of the inclined plane.

W=weight of the slide and contents.

W'=normal pressure on the plane, $= W.\frac{b}{h}$.

g=acceleration of a body falling freely.

g'=theoretical acceleration of the slide $= g.\frac{a}{h}$.

g''=the observed acceleration at any time.

Then the resistance of friction $= F = \frac{W}{g}(g'-g'')$, and the coefficient of friction $= \Phi = \frac{F}{W'} = \frac{g'-g''}{g} \cdot \frac{h}{b} = \left(\frac{a}{h} - \frac{g''}{g}\right)\frac{h}{b} = \frac{a}{b} - \frac{g''h}{gb}$ = tangent of inclination $- \frac{g''h}{gb}$.

The following tables give the results obtained from a series of four experiments. The load in every case was 40 lbs. The inclinations of the plane were as follows : No. 1=15° 6′, No. 2=16° 9′, No. 3=17° 5′, No. 4=18° 9′.

TABLE A.

Velocities.	Accelerations.			
	Expt. 1.	2.	3.	4.
4	.020	.033	....	...
10	.035	.056	.073	.092
15	.044	.070	.090	.112
20	.053	.081	.103	.129
25	.059	.083	.112	.140
30	.065	.094	.120	.150
40	.073	.105	.131	.165
50	.078	.112	.140	.176
60	.083	.117	.148	.184
70	.087	.121	.156	.190
80	.091	.125	.159	.196
90	.093	.128	.163	.200
100	.095	.131	.168	.203
110	....	.133	.171	.206
120	....	.136	.175	....

Table A shows the accelerations corresponding to different velocities in the four experiments. The units used are the $\frac{1}{1000}$ of an inch and the $\frac{1}{10}$ of a second.

Table B shows the coefficients of friction in each experiment, deduced by substituting the observed accelerations given in Table A in the formula given above. The observed accelerations were of course reduced to feet in a second.

TABLE B.

Velocities.	Coefficients of friction.			
	Expt. 1.	2.	3.	4.
4	.260	.273	....	....
10	.252	.261	.270	.280
15	.245	.254	.261	.270
20	.243	.248	.254	.260
25	.240	.245	.250	.255
30	.237	.242	.246	.250
40	.233	.236	.240	.242
50	.230	.232	.235	.236
60	.228	.230	.231	.232
70	.226	.228	.227	.231
80	.224	.226	.226	.225
90	.223	.224	.224	.223
100	.222	.223	.221	.222
110	....	.222	.220	.220
120	....	.220	.217	....

From the tables it will be observed : 1st. That with a given inclination of the plane, the coefficient of friction decreases as the velocity increases, rapidly at first but more slowly afterward. 2d. With the same velocity, the coefficient of friction is greater the greater the inclination of the plane, within the limits of the experiments. 3d. The coefficient of friction in each experiment tends toward a constant quantity. 4th. This constant seems to be the same in each experiment.

No simple expression which will show the variations in the coefficient of friction has yet been found ; indeed, I have not thought best to attempt to formulate the work till certain errors which will be referred to, have been corrected. It was found impossible to procure a plank with a perfectly uniform surface. The one used in the experiments given showed at the same inclination and velocity a coefficient which slightly but regularly increased from one end to the other. The end which gave the lower coefficient was placed uppermost. The obvious result of this was to make the coefficients in Table B at high velocities greater than they otherwise would have been. This fact also explains the apparent anomaly in columns 3 and 4 of the same table,

where the coefficients at high velocities are seen to fall below the corresponding coefficients in column 2.

In experiment 4 the slide had the velocity 120 at a distance of 40 inches from the upper end of the plane; in experiment 2 it did not acquire that velocity until it had passed over a distance of 60 inches, and consequently was on a rougher portion of the plane. The uniformity of the plane was tested by starting the slide at different points along its length, and comparing the curves on the smoked glass. These experiments have not been corrected for the resistance of the atmosphere. The effect of such a correction would be to diminish still more the coefficients at high velocities.

As the inclination of the plane increases the normal pressure decreases. Thinking that this change of pressure might explain a part of the difference due to a change of inclinations, we made three experiments at the same inclination, with weights of 18, 80 and 140 lbs. in the box. At the end of one second we found the velocities in the three cases to be as 1, 1.18 and 1.32, showing a less resistance in the case of the greater load, and corresponding to a decrease of about 2¼ per cent. in the coefficient of friction. This seems to be insufficient to explain the change in the coefficient when the inclination of the plane is changed. But it is interesting as showing that in the case of pine on pine friction is not strictly proportional to the normal pressure.

As soon as possible we propose to repeat these experiments, extending the range of velocities, also to try the effect of a change of pressure, with a view to formulate deviations from the received laws, if simple expressions can be found. We have also designed a modification of apparatus to test our results when a uniform motion is given to the slide.

BANNER'S SYSTEM OF SANITATION.

By Major H. C. SEDDON, R. E.

From "The Architect."

For the past few years there have been great advances towards remedying the evils to which the convenience of having water-closets inside our dwellings has chiefly given rise. Many have been the patents taken out for closets trapped in diverse ways, as well as for sewer traps, till at one time freedom from sewer gas was generally supposed to be in direct proportion to the number and ingenuity of the traps intervening between the sewer and its different connections with the interior of the house. Then, however, the dreadful truth was announced that sewer gas, under pressure, could, nay did, force its way through all water traps, and that the suction caused by the passage of sewage matter through the pipes frequently unsealed the best traps by drawing the water out of them. Gradually, and only very gradually, has the device been adopted of providing safety-valves, to take undue pressure off the traps by leaving the soil-pipes open at the top. It would, perhaps, not be far wide of the mark to assert that in the larger number of houses erected every year even this simple precaution is omitted; whilst generally, where it is done, a small pipe carried up to the roof of the house, for the sake of a paltry saving, is made to do duty for the whole section of the soil-pipe. Now, however, an entirely new method of dealing with our soil-pipes is being prominently brought before the notice of the public. This is known as the "Banner System," after the gentleman who originated it in his own defence, and who worked out the details which form such important features in its practical application. Mr. Banner tells us that immunity from sewer gas is not to be obtained by a multitude of traps, but rather by having but one trap. At one fell swoop he does away with all the traps upon which we have hitherto relied for safety. By means of a very ingenious trap of his own invention, fixed at the foot of the soil-pipe, he cuts off

effectually all communication between the soil-pipe and the drain below. This done, he induces a constant current of fresh air from the bottom to the top of the soil-pipe, by means of a patented wind-cowl fixed at the top of the soil-pipe, above the roof of the house.

Mr. Banner's house is situated on high ground, at a level of about 150 feet above the sea. It has a good external appearance, but, being on a terrace, is only open to the air at the back and front. At the back the ground is open, without anything to impede the view. The reception rooms are large, lofty, and well lighted, and the bedrooms are of good size and height; in all there are seventeen rooms, besides the kitchen offices in the basement. The above is all that can be said in favor of the house, for, in my opinion, one worse constructed, so far as all accepted theories of sanitary arrangements are concerned, it would be difficult to find. The water-closets are placed in the very worst positions that could be selected. After passing through the entrance hall, you come upon the inner hall, with a well staircase lighted by a skylight near the roof. Off this hall is a water-closet, with another immediately above it on the second floor, both being in the centre of the house and next to the party wall of the adjoining house, where no fresh air or other than borrowed light can reach them; and, as if to make the conditions still worse, both closets, which are only 4 feet by 3 feet, are reached by passing through closely-confined lobbies, 3 feet by 3 feet, that on the ground floor being fitted up with a lavatory basin, and on the second floor with a small housemaid's sink, and each containing a gas burner, which in winter has to be lighted at 5 P. M., causing the temperature in these unventilated boxes to range at the ceiling line from 80° to 90°, and even higher, although the thermometer outside may be below freezing point. Common pan closets, with the ordinary receivers, or storage chambers for filth, formerly discharged their contents through leaden D traps into a solid pipe trapped below and closed above, whilst the drain leading from the soil-pipe ran, as it still does, under the kitchen floor in the basement to the sewer in the middle of the road, with a very quick fall; receiving on its way, besides part of the roof water, a tributary drain from the scullery sink as well as the contents of the servants' closet, which is under the street pavement, and opening off the front area. Situated as the house is, nearly at the highest point of a branch sewer, with no ventilating pipe carried up above the roof to relieve the house traps from the pressure of sewer gas, it is not much to be wondered at that the nuisance arising from his well drained house nearly drove Mr. Banner to despair, until at last it became a question of either finding some means of remedying the evil, or of seeking refuge in flight. Mr. Banner, however, proved equal to the emergency, and although at that time ignorant of the very A B C of house drainage, determined to prove the truth of the old proverb that "necessity is the mother of invention." He set to work seriously, mastered the details connected with his own troubles, first carried up the open soil-pipe above the roof of the house, and after many trials eventually succeeded, and I say so on conviction, in constructing a trap which most effectually cuts off all chance of sewer gas finding its way into any part of the soil-pipes within the house. There was a great point gained, and seems naturally to have led up to the next step, which, as I shall explain further on, appears to me to be the most important point in the whole system, namely the outlet to the open air just above the patent trap at the foot of the soil-pipe. This became a necessity, owing to the air driven down the soil-pipe by water descending from the closets above not being able to force the patent trap as it would an ordinary syphon trap, the result being, that it had to escape through the closet pans, and other trapped passages, into the house. This apparent objection to the rigid barrier placed at the foot of the soil-pipe, no doubt suggested the idea of providing a free outlet below for the air forced down by the sewage matter in its descent. The soil-pipe running down the centre of the house being now open to the air both at top and bottom, and effectually cut off from the sewer, the crowning point of Mr. Banner's system was attained by placing a patent wind-cowl on the top. The cowl is so constructed that the wind passing through

it produces a constant draught up the soil-pipe, drawing fresh air from the garden level below, and through any untrapped inlets in the house, and so setting up a continuous counter-current in opposition to any tendency of the fires, &c., in the house to draw supplies of air through the house connections with the soil-pipe. The extracting power of the wind-cowl being once established, it became evident that the traps to the closets, sink, bath and lavatory basin were no longer of any use, and therefore, being mere obstructions and receptacles for sewage matter, were removed, leaving nothing but the patent trap at the foot of the soil-pipe to guard the way from the interior of the house and the sewer, except, of course, the water in the pans of the closets when not in the act of discharging.

Mr. Banner's patent trap, the working of which has already been described in these columns, occupies about the same space as an ordinary gas meter, and is concealed from sight by a wood casing in a recess in a cupboard, being fixed about 3 feet above the basement floor. About 6 feet above the trap are the closet and lavatory basin on the ground-floor, about 30 feet higher are the upper closet and housemaid's sink, the bath being some 8 feet higher still, and the wastes from the bath and sink passing into the soil-pipe just below the closet on the second-floor, whilst that from the lavatory basin, on the ground-floor, discharges into the soil-pipe just below the lower closet. The top of the soil-pipe upon which the extracting cowl is fixed, rises about 8 feet above the roof, and, being at the centre of the house, is not visible from the road. The communication with the outer air at the foot of the soil-pipe is formed by carrying a 2-inch pipe just above the trap, under the dining-room floor, to the garden in rear of the house. The cistern, which is above the skylight over the central staircase, supplies the water required for purposes of ablution as well as for the closets. The waste from the cistern discharges on to the lead flat round the skylight, whilst all the water running off the flat passes beneath the floor-boards of one of the top rooms into a down-pipe on the front of the house, discharging over a Dean's patent trap in the area of the basement. Here the waste from the scullery sink, and the rain-water pipe from the roof, are made to discharge in the same way over a similar trap; though why the water from the lead flat should not be turned into the soil-pipe, which is close at hand, and so be made to assist in cleansing it, instead of being carried through the floor of a room, is by no means clear. From the foot of the soil-pipe, which, it is seen, receives none of the refuse water from the kitchen, the drain runs, as already stated, under the kitchen floor and through the area, with a very considerable fall, till it joins the main sewer in the middle of the road, the total distance being about 45 feet; only receiving on its way what passes through the surface traps in the open area, and the contents of the servant's closet under the street pavement, and discharging freely into the sewer without any intervening trap or flap-valve.

Having said thus much about the sanitary arrangements of the house, I have only to add, before passing on to the result of my investigations into the practical working of the system, that no attempt whatever has been made to ventilate any of the rooms, by providing inlets for fresh air or outlets for foul air. Doors and windows do duty for the first, and the smoke-flues for the last. I consider, therefore, that if under no circumstances air has been drawn from the soil-pipe, through the untrapped openings, into the house, the extracting cowl can claim to have performed its duty thoroughly, though placed under the most adverse circumstances.

I will now describe what I saw of the practical working of the system in Mr. Banner's house, and the tests to which it was put in my presence. Beginning at the highest point, namely, the patent cowl—which Mr. Banner informed me had been fixed for over a year, without once getting out of order—I first satisfied myself that it was performing its duty properly, veering with the wind, and drawing up air through the soil-pipe; this was evident from the strong current of air passing in through the mouth of the air pipe running from the garden to the foot of the soil-pipe, as well as from a perceptible indraught through the untrapped pipes from the lavatory basin

and the closets. It was plain, moreover, that no air could be passing from the soil-pipe into the house, though certainly the general conditions of the atmosphere were favorable to the cowl, there being a fair breeze blowing and the temperature being high for the time of the year, so that the drawing power of the house fires was by no means great. As a matter of course it followed that no smell could be perceived arising from either the waste-pipes to the bath, sink, or lavatory basin, or from the closets, even when the handles were purposely kept up, although they were of the common pan description, with the usual receiver, which can never be otherwise than coated with filth inside. Passing down to the bottom of the soil-pipe, we next watched, through the glass plate which forms the front of the patent trap, the action of the cup valve within, while copious discharges were sent down the closets above. By means of a strip of glass inserted in the front of the soil-pipe, just above the top of the trap, the water could be seen rising in the foot of the soil-pipe, until it reached a height of about 12 inches, when the weight of the column of water being sufficient to overcome the resistance of the weight at the end of the lever arm, forced the valve down, discharging the contents of the pipe above into the drain below without unsealing the cup, which, directly the discharge ceased, leaving only the water retained in it when at its lowest point, closed up again, with a slight deadened sound, against the indiarubber ring on the end of the soil pipe; the sound of the trap closing after each discharge was easily detected, even in the upper closet, by anyone listening for the sign of its having done its duty. The air-tight joint upon which the weighted lever was fulcrumed was simply and carefully constructed, and can safely be relied on to prevent the passage of sewer gas. The patent trap itself formed a perfect barrier against the passage of sewer gas from the drain into the soil-pipe above, and could not by any possibility be deprived by suction of the water which alone, when it is open, guards the way; whilst if, from disuse, while the family is away, the water all evaporated, the trap would, I believe, be still a perfect barrier, and could not be opened, except by water discharged from above, which, in the act of opening the cup, would reseal it. That there is no tendency for the sewer gas below the trap to force its way through the water, which seals it when open, in order to exchange places with the sewage matter discharged from the soil-pipe, is due to there being a free passage for sewage matter and sewer gas from below the trap right away to the street gratings; as well as to the freedom with which air enters the soil-pipe above the trap, and replaces the discharged matter, without any effort to restore the equilibrium being required, as is the case with a soil-pipe carefully trapped at every point.

In order to test the efficiency of the trap under extraordinary circumstances, we passed down from the closet above some corks, hair, and a piece of an old curtain about the size of an ordinary duster. The piece of white curtain was seen to pass straight through the trap, which however did not close after it, though, of course, the water seal was maintained. Hot water was then discharged from the upper closet, whilst I stood by the outlet pipe in the garden, from which the air in the soil-pipe rushed with considerable force, but without any disagreeable odor, that I could possibly detect, though I fancied I perceived a very faint smell with the first rush which certainly would have been expected after treating with warm water the inside of a soil-pipe which had been more than twenty years in position, and which must necessarily be fouled by every discharge from a closet; though, of course, it was hardly in its natural condition, after having been washed by so much clean water as had been sent down it in the course of our experiments. The inside of the pipe, I was informed by Mr. Banner, had been found, wherever examined, to be coated to a depth of more than a quarter of an inch with a calcareous deposit not of the sweetest description, to judge from the section of a leaden D trap in a similar condition, which, though it had been removed from its place for more than a year, and left exposed to the air, still emitted a most offensive odor. In order that we might examine into the state of the cup valve and the india-rubber ring against which

it closes, the glass front to the trap was then removed, and the inlet leading from the bottom of the trap to the drain stopped with a conical india-rubber plug kept for the purpose, but not before it was unpleasantly evident that there was sewer gas in abundance ready to force its way through any defective joint, could it but find one out. The cause of the cup refusing to rise was found to be that one of the corks thrown down had been nipped, end on, between the bottom edge of the soil-pipe and the bottom of the cup, just as the latter was in the act of closing, and that the subsequent discharges of water from the upper closet had failed to release it, was owing, without doubt, to the force of the descent being broken by a bend purposely made in the soil-pipe, a little above the trap; the result being that it shot quietly through the open cup, without having impetus enough to lower it, and so to open the jaws which gripped the cork. Had it been so desired, the obstruction might easily have been got rid of, without opening up the trap, by lifting the weighted arm of the lever at the same time that the trap was being flushed from above, but that was not the object we had in view. Such a *contretemps*, I was told, was never known to have occurred before, since the trap was fixed in September 1873, nor do I attach much importance to it, seeing that it would in no way interfere with the proper working of the system, even if the cup remained down for some time before attention was drawn to it, whilst it could be set right in a few minutes without any inconvenience to the inmates of the house. The cup itself was in perfect order, and free from any solid matter (the cork excepted) beyond a slimy coating of lime, which the water deposits on all surfaces with which it comes in contact; whilst the india-rubber ring appeared to be as sound as when first put on, more than a year ago, owing, no doubt, to its constant immersion in water free from destructive agents, such as grease.

When the front of the trap had been replaced, I poured some strong scent into the mouth of the air-pipe leading from the garden to the soil-pipe, in order to ascertain whether the air passing into the soil-pipe might not, at intervals of unequal action, find its way into the house, as well as out through the wind cowl. I was unable, however, to detect the odor of the scent in any part of the house, and must therefore conclude that the suction of the wind cowl was at all times sufficient to overpower that of the house.

The result of stopping up the mouth of the air-pipe leading from the garden was shown by discharging water from the pan of the upper closet, the effect on the lower closet being that the air in the soil-pipe—finding it impossible to force the patent trap below—in its efforts to escape, first raised the level of the water in the pan, and finally burst through, sending the water flying in all directions.

The same operation was then repeated, only with the lever of the patent trap raised, so as to put it in the condition of an ordinary water-sealed syphon; the result was that the air forced the trap without repeating the commotion in the pan of the lower closet. Finally, leaving unstopped the mouth of the air-pipe, and removing the plug of the lavatory basin on the ground-floor, water was again discharged through the upper closet, in order to see whether, under such circumstances, air could be forced into the house through the waste-pipe from the basin. Such however, was not the case, but there was rather a suction through the waste into the soil-pipe. It should be further observed that, so long as the soil-pipe was open to the air at both ends, no amount of water discharged down it had any tendency to draw the water contained in either of the closet pans. I also ought to mention that Mr. Banner had just had a small louvred ventilator placed near the ceiling in the lower closet, in connection with a pipe branching out of the soil-pipe, for the purpose of drawing off the hot air from the ceiling level when the gas is burning. That the extracting power of the wind cowl could, within certain limits, be safely utilized for such purposes there is no doubt, but time had not allowed of any observations being made as to its efficiency.

This closes the account of my investigations into the practical working of Mr. Banner's system of sanitation, as far as I found it had been carried out in his

own house. I have no hesitation in saying that it worked admirably, and that he has succeeded in rendering his house absolutely secure against that most insidious of enemies, "sewer gas," provided no leakage is possible from the drain which passes under the basement floor to his patent trap.

Having already described at considerable length the successful application of Mr. Banner's system to the drainage of his own house, I propose now to examine into its claims to supersede, in whole or in part, other improved methods of treating the pipes which convey sewage matter from our dwellings. In doing this, I shall confine myself in the first instance to soil-pipes and drains, under similar conditions to those against which Mr. Banner had to contend, then pass on to the consideration of buildings designed on principles more in accordance with the requirements of the leading sanitary authorities of the present day, after which I shall have a few words to say with reference to the application of Mr. Banner's patent cowls to the purposes of ventilation in general.

First, then, let us take any house in which the closets are so located within the building that the natural inference is that they are more or less certain to prove sources of annoyance, and consequently elements of danger to the health of the occupants. In such cases I have no hesitation in saying that, if the soil-pipes are treated in the same way as in Mr. Banner's own house, all chance of annoyance from sewage gas would be cut off, provided the drain from the foot of the soil-pipe to some little distance beyond the walls of the house is rendered secure against leakage. This might be accomplished either by the pipe being well cased in Portland cement concrete rendered on the outside with half cement and half sand, or by being formed entirely of Portland cement concrete troweled perfectly smooth on the inside and rendered on the outside, so as to be in one continuous length. Such a drain should be made somewhat of the section of an ordinary oval sewer in miniature, the invert being formed first, and worked quite smooth before being covered in at the top. It is very important to continue the special mode of construction adopted for some distance beyond the walls of the house. Gas escaping from a drain-pipe has a tendency to creep along the outside of the pipe, through the slight voids frequently formed by the shrinking of the soil away from the pipe. It thus can find its way within the walls of the building, and thence ascends to the upper stories through such channels as are afforded by wooden casings to gas, water, soil, or rain water pipes, void spaces behind the lath and plaster on battened walls, &c.; and when it reaches the different floor levels it finds a free passage through the spaces between the floor joists to any rooms that happen to require supplies of air to make good what is being continually consumed and carried up the chimneys by the draught of the fires. However, in order to prevent any annoyance from a closet, it is not sufficient to ventilate the soil-pipe and exclude the sewer gas from it. The proper ventilation of the closet itself is also a necessity; not merely for the purpose of carrying off the heat and fumes arising from gas burners, but also to keep the smell given off from the closet basin, before its contents are discharged, from being perceived in the house. In order to effect this, a small shaft carried up from the closets to the roof of the house, with one of Mr. Banner's patent cowls at the top, would be of the greatest advantage. It would insure a constant up-draught, which can never be depended upon, under all conditions of the atmosphere, unless there is some constant active force at work, such as the cowl would supply, or such as can be obtained by taking advantage of a heat generated in a kitchen flue, supposing one to be available for the purpose.

As the Banner system thus offers a safe way of escape from the evils which are so generally the result of placing closets in the centre of a house, it follows that, where the system is adopted, architects will be, to a certain extent, relieved from the necessity, which has hitherto been felt, of sacrificing the most convenient positions for the closets for the sake of cutting them off from too intimate a connection with the interior of the house. But, while we acknowledge the efficiency of the system as placed before us, we are bound to ask ourselves whether it is at present found in its best

and simplest form for general adoption; whether, in fact, all the details connected with its first introduction to our notice are essential to the success of the principles involved. It is one thing for all the parts to go on smoothly in the hands of those who understand and take an interest in such appliances, whereas it is quite a different matter when we keep in view the ordinary householder's indifference, and ignorance of sanitary matters, and the tricks which children and servants contrive to play upon everything within their reach. What would be admirably adapted for the mansions of the wealthy, public buildings, important offices with responsible care-takers, and the homes of those who live in houses of their own, would not be suitable for houses of an inferior description, or for those which are perpetually passing through the hands of tenants. In order, therefore, to enable us to arrive at some definite conclusions on the subject, I propose to consider the following points, which appear naturally to present themselves for discussion.

1. If we admit that the extracting cowl will act efficiently at all times and under all circumstances, does it not follow that the trap at the foot of the soil-pipe might be removed altogether, and the latter left fully open to assist in ventilating the sewer, in addition to the other duties it is called upon to perform? This is a point which has been put forward more than once, and one which, at first sight, seems only answerable in the affirmative. If the cowl at all times and under all circumstances draws air up the soil-pipe with such force that no air or gas can escape out of the latter, except through the cowl, there does not seem to be any reason why we should place a trap between it and the sewer. Why not let the soil-pipe act as a natural outlet for sewer gas? The only answer I have heard given to this question is, "Well, no doubt you might safely dispense with the trap at the foot of the soil-pipe, but it is not pleasant to know that gas from the public sewer is passing up a pipe within the walls of your house. If the same plan was pursued in every house, well and good, but one does not like ventilating the sewers for the whole community." Such an answer, however, will not bear investigation; for, if a principle is correct, no amount of sentiment ought to stand in the way of its being carried into practice. I think, however, there are sounder reasons for maintaining the separation between the soil-pipe and the sewer. One is that, though the cowl may always be efficient, the pipe below it may, perchance, get stopped up, and so allow the sewer gas to accumulate below the point of stoppage, and thence to force its way, or be drawn into the house, through traps or defective joints, and such a contingency must be guarded against at any cost. Another, and a yet more important reason, is that if the soil-pipe is used to ventilate the sewer, the lower end of it must not communicate with the outer air, and if this point is given up, all the benefit to be derived from Mr. Banner's system is given up with it, leaving us still liable, at each discharge of a closet, to have sewer gas forced into our dwellings. It is therefore clear that a trap at the foot of the soil-pipe is necessary, and ought not to be dispensed with; whilst, if the sewers are to be ventilated, a special pipe should be used for that purpose, which should be kept outside the house, and not be allowed to pass through it, for several reasons which need not be gone into here.

2. Taking it, therefore, as an admitted fact that the soil-pipe must be cut off from the drain into which it discharges, and allowing, for the sake of argument, that Mr. Banner's lever trap is to be the one used (the term "*lever* trap" is used to distinguish it from Mr. Banner's last patent, which is "double dip trap"), the next question which arises is, "Would it not be advisable to have some sort of trap close to each closet pan, or other inlet, for the purpose of arresting such articles as brushes, sticks, stones, and the various matters which any plumber will tell you are constantly causing the stoppage of traps and soil-pipes, and necessitating the aid of his valuable services?" This question I think Mr. Banner himself would answer in the affirmative, in the interest of his own trap, as any solid matter finding its way into the bottom of the copper cup would weigh it down, before the water could rise to the ordinary height within the pipe above. The result would be that the descending water would shoot through the

open trap with much less force than usual, and would not, therefore, have a fair chance of dislodging any weighty matter in the bottom of the cup; and this would effectually prevent it from closing, if such extra weight in the cup caused a preponderance in excess of that due to the weighted arm of the lever, which is so balanced that it only just retains sufficient righting force when the cup contains nothing but its complement of water. That such articles would frequently find their way into the lever trap, if fixed in ordinary houses, cannot be a question admitting of any doubt; and, when it did happen, the glory of the trap would depart from it, for it would be reduced to the condition of a common dip trap, sealed with, perhaps, not more than half an inch of water. It is true the defect could be remedied with ease in a few minutes, but who would be likely to observe it, for no stoppage would proclaim the fact that anything was out of order; and to suppose that any one in an ordinary house could be relied upon to keep up a series of observations on the height of the lever arm, in order to see whether the cup closed properly, is entirely out of the question. I am afraid that if once the cup got fixed open, it would, as a rule, remain so from one year's end to another. It seems, therefore, clear to my mind that, in order to put any such contingency out of the question, an ordinary syphon trap (certainly not any such abomination as a D trap), is required, wherever the patent lever trap is used, to act as a catch-pit beneath each closet pan, and that all wastes leading into the soil-pipe should be fitted with gratings to prevent the passage of solid matter.

3. This decision brings us to the next point, viz.—If, with a patent lever trap at the foot of the soil-pipe, we ought still to fix traps to the closet basins, the argument that the extra cost of the one trap is in a great measure met by all other traps being dispensed with, falls to the ground; and we are naturally led to ask ourselves whether a properly constructed syphon trap would not be just as efficient as the patent lever trap. In my opinion it would, and Mr. Banner has practically answered this question in the affirmative, having just patented what may be termed a "double dip, ventilated trap," to be used by those who, like myself, would prefer it to the automatic action of the more costly lever trap. This last trap of Mr. Banner's invention has, I understand, given great satisfaction in his brother's house, and it has the same advantage as his first patent in allowing the working of the traps to be seen through glass plates fixed in its movable face. The outlet from this trap for the ventilating pipe to be carried above the roof of the house, however, appears to me to be unnecessarily large. As the ventilating pipe will in most cases have to be carried up through the house itself, I should have made it much smaller, treating it merely as a safety valve, to prevent any suction unsealing the dip traps; for the ventilation of the drain itself I should much prefer carrying up an independent pipe, outside the walls of the house. The benefit to be derived from the two dip traps, one immediately below the other, is also I think more than doubtful; whilst the fall into them is, to my mind, too direct, and the chances of stoppage thereby increased. For my own part I should be perfectly satisfied pith a common 6-inch syphon trap constructed with a gentle sweep into it from the 4-inch soil-pipe, to enable the sewage matter to shoot the trap, instead of falling dead on the surface of the water within it, and provided with an inspection hole placed near the highest point of the trap, instead of at the bottom of the bend, where it helps to arrest everything passing through the trap. Such a trap does not require any safety valve to prevent its being unsyphoned, for the simple reason that the 4-inch soil-pipe can never make the long leg of the 6-inch syphon run full; nor could Mr. Banner's trap be unsyphoned, even if he omitted the ventilating pipe altogether, provided he took care that the outlet from his trap was considerably larger than the inlet to it. With either of these traps there would be no necessity for the catch traps below the closet basins, which could therefore be omitted, as their only object was to prevent the lever trap from being reduced to the condition of a mere dip trap; thus, the simpler the means we employ, the fewer parts we seem to require, and this without in any way affecting the efficiency of the whole system.

4. The last point to which I shall refer in connection with Mr. Bannér's treatment of soil-pipes from closets placed in the centre of a building, is the following —granted that his extracting cowl is very perfect, powerful, and constant in its action, and that when used on the top of a soil-pipe, ventilated at top and bottom, and trapped with a syphon at its point of junction with the drain, all other traps may be dispensed with; taking all this as proved, cannot we carry out the system without the aid of the cowl? My answer to this would be, "certainly, provided you choose to trap the wastes from your sinks, baths, and lavatory basins, and use valve closets of good construction, in place of those receptacles of filth which go by the name of pan closets;" at the same time, I know that Mr. Banner considers that it would not be safe to have the inlet at the bottom of the soil-pipe, without an extracting cowl at the top. In my opinion, by simply allowing the soil-pipe free communication with the outer air, both at the top and bottom, it will be thoroughly ventilated, whilst the traps to the waste-pipes, and the water in the basins of the valve closets, will sufficiently guard against any chance of the suction of the house, even if the rooms are entirely unprovided with proper inlets for fresh air, drawing air from the soil-pipe, provided the pipe is sound, and the joints tight. True, you will not have the suction of the cowl to remedy any tendency for the air in the soil-pipe to enter the house, in the event of the soil-pipe being in any way defective; but, with ordinary care, I do not think there would be sufficient reason for our fixing extracting cowls to all soil-pipes, with the object of guarding against so remote a contingency; though, in large mansions, and other important buildings, such extra precautions might be taken with advantage. That the air in the soil-pipe would be constantly renewed, and, in fact, in a constant state of motion, without the aid of the extracting cowl, must, I think, be evident to all. It is clear that every discharge from a closet would drive out at the foot of the soil-pipe all the air below the level of that closet, and that fresh air would follow in the wake of the descending water; the air in the branch pipe, leading to the soil-pipe, being replaced by air following it through the closet valve—no trap being placed between the valve and the soil-pipe—and the air in the soil-pipe being replaced by fresh air descending through the open top. Moreover, the different conditions of temperature above, below, and within the soil-pipe, arising from various causes, such as the sun warming the top of the pipe above the roof, and the warmth of the house affecting portions of the pipe running through it, would tend to keep up a constant movement of the air within it.

I will now pass on to consider a more favorable condition of affairs, such as would occur in designing a new building, where there are no special circumstances to compel the architects to place the closets in other than what is acknowledged to be the best position for them, namely, against an outer wall, where light and ventilation are at full command. In such a case it is always desirable to enter the closet through a freely-ventilated lobby, with cross ventilation where practicable, and so arranged that under no circumstances can the inlets and outlets be entirely closed. The soil-pipe from the closet should be carried down outside the walls, and, therefore, the junction with the drain being outside the house, no danger can arise from sewer gas finding its way inside the dwelling, except through the branch pipes from the soil-pipe, and this cannot occur if a properly constructed syphon trap is placed at the foot of the soil-pipe; more especially if means are taken to prevent any pressure of gas on the sewer side of the syphon, by carrying up a pipe therefrom to ventilate the drain. Mr. Banner's traps would, it is evident, under such conditions be quite superfluous, as special receptacles would have to be constructed for them outside the building—though, in some cases, I am aware this has been done; whilst, as already pointed out, with the soil-pipe open both above and below, quite sufficient ventilation would be obtained without the use of the extracting cowl, which, in any prominent position, would certainly not add to the appearance of the building. From the closets themselves it would be a great advantage to lead up an extracting shaft, surmounted by a patent cowl, in order to remove at

once any unpleasant odor arising from the basins, before the contents are discharged into the soil-pipe.

It will be gathered from what I have already said that, although I look upon any house, treated as Mr. Banner has treated his own house, as perfectly secure against all danger arising from the unhealthy emanations of sewers and their necessary adjuncts, I do not consider the patent trap or the patent cowl essential parts of his system, but that its great claim to attention lies in the thorough ventilation of the soil-pipe, by letting the outer air pass freely in and out at the bottom as well as at the top. This point admitted, I hold that it will be well to dispense, as much as possible, with all contrivances depending on the free motion of working parts, and which involve the wear and tear of mechanically-made joints. That this can be done without running any risk of allowing foul air or gas to pass from the soil-pipes into our houses, I think I have sufficiently demonstrated. If properly constructed valve closets are used, there will be no necessity for any trap between the valve and the soil-pipe. The way should be perfectly clear, and each discharge will then suck air down after it, none will come up through the valve; whereas, where a trap is used, the air between the trap and the valve must make room for the contents of the basin, which it does in the readiest way, namely, by exchanges places with the latter, and so causing that offensive puff of foul air, which is always the result of pulling up the handle of an ordinary closet.

The wastes from baths, basins, and housemaid's sinks should, as at present, be trapped; those from scullery sinks should discharge, if possible, over a trap in the open air, and such trap should be readily cleaned out. Dean's patent trap, which has an iron bucket inside, is about the simplest and best I know of, as the bucket can be raised, emptied, rinsed out, and replaced with the greatest ease, and without hardly soiling the fingers. The safety-pipe carried up from the trap at the foot of the soil-pipe to the top of the house should never be omitted, unless there is some outlet to the open air—such as through a gully grating, not far distant from the trap.

The last remarks I have to make are on the proposed application of Mr. Banner's patent cowls for the thorough ventilation of houses and sewers, in addition to the soil-pipes, as set forth in his pamphlet entitled "Wholesome Houses." As the suggestions of the patentee have not as yet been put to a practical test, there are no ascertained facts to refer to, but I should strongly recommend the three up-cast pipes for extracting the contents of the rooms, the sewer and the soil-pipe, not being united together and submitted to the action of a single cowl; as nothing is accurately known with regard to the amount of work such cowls are capable of performing under different conditions of wind force. It would be much safer to use a separate cowl for extracting foul air from the rooms, whilst in the rooms themselves proper provision should be made for supplying fresh air in place of that carried up the smoke flue and drawn off by the cowl, otherwise the stronger of the two will draw air down the other, and smoky chimneys may have to be cured by removing the cowl from the summit of the foul air pipe; or, if both smoke flue and cowl managed to draw air out of the room, the draughts of cold air from the doors and windows would bring discredit upon ventilation, and make people sigh for the days when every one lived in comfortable ignorance of the reason why their sojourn on earth fell so far short of the 969 years of Methuselah. The advantage of the inlet for fresh air, at the bottom of the pipe for extracting foul air from the rooms, is by no means clear, since this pipe does not require to be kept fresh by a constant current of pure air passing through it; and I take it to be quite a mistake to imagine that the fresh air drawn through the pipe by the cowl has more power in drawing air off from the rooms than the direct action of the cowl would have on the outlets from these rooms. For all purposes, however, of insuring the proper action of foul air shafts, especially from water-closets, hospitals, prison cells, &c., I think these cowls will prove of the utmost value. One of the great difficulties the sanitary engineer has to contend with is the liability of his outlet shafts, under certain conditions, to play him false and act as inlets, and so to reverse the whole of his carefully arranged sys

tem of ventilation. For smoky chimneys I know that they have proved most successful, in cases where almost every known contrivance had previously been tried without success.

The proposed application, however, of these extracting cowls to the ventilating pipes carried up to the roofs of houses from the drain pipes, as advocated by Mr. Banner in his pamphlet, so as to draw constant currents of air through the sewers and street drains, appears to me to be founded on a principle which will not bear investigation. That such outlets for the gases produced in the sewers ought to be provided, there can be no doubt; and I will go further, and say that there ought to be power to compel everyone who builds a house in future to make such a provision; but I think that such outlets should be regarded as safety valves, and should be allowed to do their own work, without the assistance of any extracting cowls, which would simply aggravate the evil, and turn our sewers into great underground gas manufactories. You do not want to promote decomposition, and so to increase the production of sewer gas, but merely to prevent the dangers arising from its being pent up in the drain pipes. Those who know how lighting gas is produced, by merely drawing or forcing air through underground tanks of petroleum oil—where, in passing over the surface of the oil, it picks up the vapor which enables it to burn with such a brilliant light—will see, at a glance, what will be the effect of drawing streams of air through sewers and drain pipes, and discharging them perpetually into the atmosphere, to be blown into our houses, and to contaminate the air in our streets. We must not strain after too much, a sewer must always be a thing of dirt, and there is no necessity to turn it into a desirable place of retreat from the heat of summer, or the cold of winter. Already we are told by analysts that the air in some of our London sewers has been found to be quite as pure as that which the fashionable world are content with in many of their haunts of pleasure, and, if we can attain to such a standard throughout the whole of them, we need not mind if the street gullies sometimes act as outlets, and the ventilating pipes up our houses as inlets. If we only provide the requisite openings, and construct our drains and sewers properly, the intermittent flow in such underground channels, driving out air and sucking it in, as well as the never-failing law of nature, in obedience to which the lighter gases must rise and give place to the heavier, will keep the air within them in such a constant state of motion that all the evils which are laid at the door of the water carriage system of house drainage will vanish, and we shall wonder that it could ever have been seriously suggested to relieve the waste water from the duty, which it so cheaply performs, of carrying our solid sewage far away from our houses and towns.

Before concluding, I must say, in justice to Mr. Banner, that having no professional knowledge of sanitary engineering or sanitary science, the greatest possible credit is due to him for having succeeded in solving a problem which has hitherto baffled all the combined talent of the best sanitary authorities of the day; and that, whether we choose to adopt his system in its entirety or not, he has certainly taught us something that, however well it was known before in theory, no one has been equally successful in applying in the more valuable form of practice.

ARMOR-PLATES AND PROJECTILES.

From "Iron."

EXPERIMENT was made recently at the great works of Charles Cammell & Co., at Sheffield, which, it is believed, will have an important influence upon the future of our own and other ironclad navies. It was the rolling of the thickest armor-plate which has ever been produced. It marks another stage in the almost endless controversy in which the penetrating power of guns is set up against the resistance of armor-plates. Four and a half inches is the thickness

of the plates with which vessels of the *Warrior* class are covered. Step by step the size has been increased till it has reached 14 inches, which, until then, was the thickest plate known. Messrs. Cammell & Co. have now succeeded in producing one of 22 inches, this being eight inches thicker than any armor-plate ever yet rolled. The plates, of which this is a sample, are intended for the *Dandolo* and *Duilio*, two war vessels now being built in Italy for the Italian Government—one at their dockyard at Castellamare and the others at La Spezzia. These vessels are to be armored at the water-line with plates of this thickness, and the representative plate rolled lately was ordered from C. Cammell & Co. for the purpose of ascertaining the relative resistance of plates of this enormous thickness compared with the thickest that has yet been manufactured. The gun to be used in testing this great plate is one of the 100-ton guns now being made by Sir William Armstrong & Co., at Newcastle. The vessels are to have two turrets, and each turret will contain two of these enormous pieces of artillery. The guns will be about 30 feet long, their bore 19 inches in diameter, and they will throw a shot weighing nearly one ton. Several hundred pounds' weight of powder are necessary for each charge. One of the guns is nearly ready, and Sir W. Armstrong has been specially asked to make a crane capable of lifting 150 tons to move it. To give some idea of the enormous mass of metal of which the plate is formed, it may be stated that it had to be in the furnace upwards of twenty-seven hours before it was fit to be placed upon the rolls. It weighs upwards of 35 tons, and measures 17 feet in length and 5 feet in width. The experiment of rolling such a monster was a bold one. Sir Joseph Whitworth, Sir W. Palliser, and a number of officials and diplomatists were present to witness the operation.

Before the plate was rolled, a luncheon was served at the works, at the conclusion of which a few toasts were given and responded to. Sir Joseph Whitworth's health was proposed in connection with his guns. In giving it, Mr. Cammell stated that if Sir Joseph's guns succeeded in penetrating the plate about to be rolled, he should have no hesitation in rolling one of 30 or even 40 inches in thickness. In reply, Sir Joseph Whitworth kept significant silence with regard to what he believed his guns would do when opposed to a 22-inch plate. Sir William Palliser's health was also given. In replying, he said, that owing to the success of his projectiles, he at first thought that the days of iron-plated vessels were numbered, and that we should return to unplated ships with heavy guns. Subsequent experiments, however, satisfied him of the enormous resistance which armor-plates presented to projectiles, unless they happened to strike obliquely at right angles, and it was this enormous resistance that, in his opinion, rendered the retention of iron-clad ships necessary to the country. Nobody could yet say whether the gun or the plate would win. If Sir Joseph Whitworth made a gun that would penetrate even a plate 22 inches thick, then a plate must be made that it could not penetrate; in fact, the bigger the guns, the more powerful must be the plates. Nobody could deprecate more than he the idea that, because of the increase in the power of penetration of our guns, iron-plated ships must be abandoned. What they required was that their plates should be more powerful. It was only in direct firing that the greatest penetration had been obtained, and it was but fair to presume that in actual warfare the greatest portion of the shots would be fired obliquely. He was quite aware that Sir Joseph Whitworth had invented a shot which would "bite" when fired from an oblique position, but even then the penetration was much inferior to that obtained by a direct shot. That being so, he was inclined to think that armor-plated ships would always possess an advantage over guns.

Shortly afterwards an adjournment was made to the armor-plate mills. A group of men were standing round the furnace in which the plate was being heated, and at the word of command from a superior they began to pull away the bricks at the mouth of it. Instantly the flames leaped out, and the men, accustomed as they are to stand a great heat, were constrained to retreat until the fury of the flames had subsided. Then one wearing only trousers and a shirt

approached the furnace, raised a little doorway, and looked at the huge monster within. The view was doubtless satisfactory, though how any one could look into this furnace unscorched was a marvel. Men were then seen guiding up to the mouth of the furnace a huge pair of tongs with which the plate was to be grasped. A trolly, too, was sent almost up to the mouth of the furnace, which, by and by, received the plate when the tongs had done their work. Everything was now ready. The door-way of the furnace was lifted up, the flames shot out and lit up the mill, and while spectators shielded their faces with their hats or handkerchiefs, the workmen, with their backs to the furnace, pushed up the tongs until they grasped the plate within.

Balks of wood were then put on each side of the furnace to enable the plate to be drawn out the more readily; but the flames seized upon them and appeared to devour them as if mere shavings. There was no time to lose, the order was given, and the machinery began to move, the chain fastened to the tongs, slowly tightened, and the huge mass, which had required twenty-seven hours in such a furnace as this before it was "done," made its appearance. Fierce as had been the heat before, it was now ten times greater. One could look upon the plate, white with heat, over and around which little blue flames appeared to be lingering. Slowly it fell upon the trolly, the tongs were then removed, and in a moment or two the rolls, which had been revolving for a while, caught the end of the plate and the huge mass, weighing 35 tons, passed between them with as much ease as if it were but a 4½-inch plate. Backwards and forwards it came six or seven times, each time the distance between the rolls being decreased, and the operation ended as soon as the required size had been attained. The rolling was most successful, and it is believed the plate is without a flaw. The destination of the plate is Spezzia, where the test is to be carried out.

The experiment shows that there is absolutely almost no limit to the thickness at which armor-plates can be made. It was no idle boast on the part of Mr. Cammell when he said that if Sir Joseph Whitworth's gun penetrated this plate he would make one 30 or 40 inches thick. The result of the test at Spezzia will be watched with great interest.

HARDENING AND TEMPERING GLASS.

From "English Mechanic."

THE well-known Dresden manufacturer, Herr F. Siemens, has recently patented a method of hardening, tempering and pressing glass, which appears likely to become of more practical utility than the process of hardening discovered by M. de la Bastie. At the time when the latter's discovery was made public it was announced that Herr Siemens and others had been experimenting in a similar direction, and the accusation was freely made that Bastie's process was being pirated. Whether or not it is so in Germany, the process described by Herr Siemens in his English specification certainly keeps clear of the "claim" put forward by M. Royer de la Bastie, for he does not employ the method of hardening glass by plunging it when heated into a liquid bath at a lower temperature, but hardens the glass by placing it in moulds and pressing it at the same time. It was obvious at the time when M. Bastie's process was made public that great if not insuperable difficulty would be experienced in getting the glass to retain its shape during the hardening operations, especially so with articles that required to be shapely to be of any value at all. Whether these difficulties have been overcome we are not informed, and as there are already several patents relating to hardened or so-called unbreakable glass in the field, unless M. de la Bastie speedily occupies a position in the market he may find himself forestalled. At any rate it is asserted that the process patented by

Herr Siemens effects all that could be done by M. de la Bastie's, and more, for the shape of the articles, can be easily preserved uninjured. It consists in a method of heating and then suddenly cooling the glass to be hardened or tempered; but when the articles are such as are usually moulded, the hardening and tempering are accomplished at the same time as the pressing—*e. g.*, the molten glass is run into suitable moulds and while still highly heated is squeezed, the moulds having the effect of giving the necessary cooling without resorting to the liquid bath of M. Bastie. The material employed for the moulds depends on the nature and thickness of the glass; in cases where the cooling process must necessarily be a rapid one, metals of good conducting power, such as copper, are preferred, while in those where the cooling has to be effected more gradually moulds of earthenware, or other bad conductors of heat are employed. In cases where the glass articles to be operated upon vary in thickness the conductivity of the parts of the moulds is varied accordingly, either by means of thicker metal near the thicker parts of the glass, or by making those parts of the mould of a better conducting material than the parts next the thinner portions of the glass. The moulds, too, must be kept at certain temperatures varying according as the nature of the glass requires that they should be cooled to a greater or lesser degree. In ordinary practice, however, it is found that cast-iron moulds maintained at a temperature of boiling water or thereabouts, and earthenware moulds kept quite cool, yield very satisfactory results. The liquid glass may be conveyed direct into the moulds, or may be taken from the melting-furnace on the blower's pipe and shaped in the mould, but it is preferable to heat the articles after shaping or partial shaping before pressing and cooling them. This part of the process introduces the difficulty of keeping the articles in shape, and it is overcome by Herr Siemens by means of casings or shells of platinum, such shells being transferred to the mould with the glass to undergo the pressing and hardening process. The heating ovens may be of any suitable construction, but Herr Siemens prefers to employ regenerative gas muffle ovens, heated under the floors and over the crowns by the flames of gas and air, which pass from one set of regenerators to another, which latter becoming sufficiently heated the currents are reversed in the well-known manner of alternated working. The muffle being completely closed in, the articles are protected from dust and other impurities which in the open furnace are apt to settle on and damage the surface of the glass.

The lower halves of the moulds are mounted on trucks or hand carriages, and are run up to the furnace mouth or the oven, as the case may be, and having received the glass are run under the respective upper halves, which may be loaded to give the desired pressure. The temperature of the moulds is kept at the required point by supplying them with liquid, and water at the boiling point is found to be well suited for the purpose. Herr Siemens claims the process described of producing hard pressed glass by treating it whilst heated in moulds at a lower temperature, whereby it is simultaneously compressed and hardened. He also claims the use of moulds having parts of varying thickness, or of different materials having various degrees of conductivity. A separate claim is also made for the use of the platinum moulds to maintain the articles in shape whilst being heated in the muffle. Whether the process of M. de la Bastie or that of Herr Siemens yields the best results no evidence is at present forthcoming, but there can be no question that a large demand will arise for unbreakable glass as soon as it can be supplied.

It appears from an official return that, during the year 1874, there were built in London 7,764 new houses and 145 new streets, and two new squares were formed, the length of the new streets and squares being 22 miles and 862 yards. 3,542 new houses were in course of construction. The length of new streets and squares opened during the last quarter of a century is 1,181 miles and 54 yards. "There does not appear," adds the return, "to be any immediate prospect of a cessation of growth of buildings; the tendency is rather the other way."

GEOGRAPHICAL SURVEYING.

By FRANK CARPENTER, C. E.

Written for VAN NOSTRAND'S MAGAZINE.

LET it be supposed that we, that is, the reader and the writer, have received commission from some state or national government to organize and prosecute the geographical survey of some portion of its wild and and unmapped domains; in this thesis the writer will submit to the reader a project for the execution of the same. Whether this territory lie in Australia, Arizona, or Alaska, will affect us only in the material of our outfit, and not in the methods of our work, which are the same wherever physical laws prevail and the face of Nature is wrinkled with mountains and valleys and furrowed with the river-bed and cañon.

In this problem, we have given us a certain sum of money, a limited period of time, and a stated area of territory of which we are required to furnish the most accurate and impartially complete map that our means will allow. The work is peculiar and difficult. It is a branch of our profession for which no training-school prepares its student and no text-book can instruct him. This is a field in which the experienced topographical engineer, fresh from his labors on park, harbor, and landscape, finds himself unhandy and incompetent, for much of the experience and tradition that he brings with him is an incubus to retard him. To become efficient in this new service, he must forget much of the rule and routine that he has learned, accustom himself to taking broad and bird's-eye views of the country, and, strange as it may sound, he must make it a matter of duty and pride to neglect much that is near at hand, remembering that, although a mole-hill at a distance of a few feet, subtends a greater visual angle than a mountain as many miles away, yet it is the mountain, and not the mole-hill, that deserves delineation on his map. Whereas he has been local and narrow in his range, he must now become geodetic, else he will accumulate a mass of minutiæ, whose representation would be infinitesimal on a plot of the proposed scale, and which is hence but an incumbrance to his books, and worse than cumbersome, inasmuch as its presence excludes other and more valuable data. In short, the topographer views the earth as a microcosm, but the geographer is a man of no such narrow horizon, and trains himself to look upon it as a macrocosm.

Of scarcely secondary importance to the men of our corps are the instruments with which they shall work. The tools which have been devised for the ordinary surveys of land and landscape must be left at home with the slow and tedious methods from which they cannot be divorced. In a work of geographical extent, the level, chain and tally-pins, are out of place, and whosoever, making accuracy his plea, attempts to introduce them there, finds his own ends defeated by them. Once upon a time, for instance, an engineer was intrusted with the survey of a large tract of new territory. The time and resources assigned him would allow him to touch the country but lightly and by swift marches, but, as this was intended to be only a reconnoisance, nothing more was expected of him than to trace the conformation of the land in a general way. He was an honest and conscientious engineer, and so great was his zeal for accuracy, or nicety rather, that he was scrupulous to a fault. He employed the spirit-level in determining the heights of stations along the route, whose distances were found by stadia measurements, which system, though considered incautiously rapid in topography, is too laggardly slow for geography. In this manner he crossed his territory with a few lines of march whose profiles were as trustworthy as those of a railroad survey, and far more accurate than the public interest demanded, while between them there were areas untouched and unseen, and of these the public, whose agent he was, had commissioned him to obtain information. The failing of this engineer is a common one; he neglected

to distribute his resources fairly and impartially, and while half of his map is reliable the other half is conjectural.

To enumerate and describe in detail all of the instruments whose use is peculiar to the making of geography, and to rehearse the devices employed in its successful prosecution would be material for a volume; however, a few of the most necessary and novel features will be noticed in the course of this paper. At the basis of the work is the transit, or theodolite, which, with compass attachment, is the engineer's *vade mecum*, without which his occupation is gone, no matter in what field his labor may lie. As an appurtenance to this, not the chain nor the stadia, but the odometer wheel, has become the recognized means of mensuration in the meander of streams and the determination of those distances of route and detour which are so useful in filling in a triangulation chart. Instead of the level the cistern barometer gives the heights of mountains, mines, passes, camps, settlements, and other important positions, while the aneroid barometer, portable as a watch and as easily read, will tell the altitudes of minor points and give with sufficient closeness the data from which may be plotted the profile of the odometer's itinerandy.

These are the three classes of instruments that are indispensable; to carry and operate them three men are necessary, the topographer, the meteorologist, and the odometer recorder. Any addition to this number, except as military escort or muleteers and servants, or as an executive officer who shall also be purveyor and guide to the marching party, would be superfluous, as one surveyor can see as far as two, and one man is able to note all of the topography visible from his route of travel. No axemen are needed, for if there is a tree in the way the line must yield to the tree; the resultant error will be trifling, and will not be apparent in a map which represents several miles of territory on one inch of space. Neither is there any necessity for rodmen, with rods of two targets for micrometer measurements or one target for levels, who retard the corps by the long delays consequent to their change of base from stations aft to front; as it travels this party is a unit, moving as fast as their animals can walk, and is never broken, a consideration which is of value in a country of hostile people. Of course the scope of the work may require the service of a great number of professional men, but its best progress demands that they should be divided into corps of the above size, which shall work in concord and under one general head, making rendezvous from time to time for the comparison of notes, reorganization, exchange and issue of supplies, etc.

Guided by these thoughts, let us suppose that we have completed our organization for a season in the field and that we are now on the ground ready for work, at the place selected as the initial point of the survey. As with all surveys, this one will be executed from stations, meaning thereby any points at which a tripod is planted and an instrument adjusted, angles are read and sketches are made. Of these we shall occupy four classes, of which, in importance, and consequently in accuracy, the astronomical is first, and then come the geodetic, or primary triangulation station, the topographical station, which is an apex for subordinate series of triangles, and finally the meander, or route station. These with the incidental details pertinent to them, will be considered in the order named.

Since the positions determined by triangulation, or by any system of survey in which terrestrial objects alone are considered, are only relative to each other and to the first station occupied, it is evident that a map may be completed, which, *per se*, will have all of the exactness of perfect truth, but whose place on a projected surface of the globe will still be uncertain. A map of a continent may be made, and this may be of great use in the guidance of travelers, across the continent, and for the local information of its inhabitants, but still it does not play its proper part in the grand plan of this earth's geography, and define the situation of this land relative to the other continents of the earth, until it is bound into place by the meridians and parallels, which are the warp and woof of the structure of geography. Therefore, in order to adjust our map, when made, into its true place, we must

have the absolute determination of one or more of its positions.

Now there is but one way of finding the absolute position of an object on the earth, and that is by going beyond the earth, consulting the stars, and ascertaining its place relative to them. Having two triangulation stations thus located the whole chart becomes adjusted to its place. Or, having the latitude and longitude of our initial point and the astronomical azimuth of the side of a triangle leading from this origin, the former serves to pin the plot to the projected map, and the latter is instrumental in orienting it into the area to which it belongs.

It is probable that our point of outfit is on the verge of some civilization, and perchance it is connected by telegraph with some national observatory, whose longitude is known. If so, a series of exchanges with that observatory and a series of observations for latitude, extending through a couple of weeks, will, in auspicious weather, be enough to determine with sufficient accuracy the geographical co-ordinates of our point of departure. This can be done by the astronomer while the engineers are measuring the base-line and developing the same, the director is perfecting the organization, and the purveyors are distributing the instruments, supplies, and all of those numerous articles of equipment which are the furniture of a scientific field season. At the same time the meteorologist, by a set of hourly barometric readings, accumulates data whose digest will give the vertical co-ordinate of this place with a possible error of a very few feet, and this completes the determination of its position with reference to a system of three axes whose origin is at the level of the sea at the point where the first meridian crosses the equator.

An inland survey, based upon trigonometrical methods, progresses most successfully from an initial point concentrically outwards. The most fortunate location for this initial point is in the centre of some broad valley or intermontane plateau, whose level expanse offers fair ground for the measurement of the base and whose open field is favorable for the gradual and symmetrical development of the same until it shall reach the lines of the remotest triangles, in which it becomes a metrical standard for finding their length. In an extensive survey, lasting for years and covering broad territory, a series of bases are indispensable. These act as checks upon each other, and the net-works of triangles emanating therefrom, are dovetailed into each other, and, in their adjustment to fit each to each, what little of error may have accumulated is reduced to a minimum. For instance, on each side of a continental divide there is an open basin. In each of these an astronomical station is established and a base is measured. On the comb of the intervening sierra, one hundred miles apart, stand two preeminent mountain peaks. The latitude and longitude of each of these, with the distance between them, is determined from the two origins independently. They check each other, and this long line, drawn by the labor-saving appliances of trigonometry through a hundred miles of aerial route, a mile above the valleys and chasms which it spans, joining the peaks like a fine spider's web, is now ready to be used as a new base in the primary triangulation.

The ground upon which a base-line is to be measured should be as smooth and bare as possible; whether it is level or not is a matter of secondary importance, as correctness may be easily applied to cancel the effect of its gradients. In its direction and position it should bear judicious relations with certain knolls, knobs, or buttes in the vicinity, which may be selected as sites for the stations to be occupied in its development, the plans for which should include some two prominent peaks in the horizon, remote from the origin and distant from each other. Owing to great exposure to the wind, or to inconvenience of approach, it may not be found practicable to locate the astronomical station at any of the points of the triangulation system, or, to secure proximity to the telegraph, whose office may be hidden in the heart of a town or the bottom of a valley, it may be so secluded as to be invisible from those points. If so, it may be easily connected with them by running a careful traverse from the astronomical station to the nearest geodetic station, and, from the results of this, computing their difference of latitude and longitude.

The length of the base may vary from one to five miles. In the opinion of many engineers more than two miles of measured length is zeal gone astray, for the advantages of accuracy gained by such excess would be obtained more easily by devoting the extra time to a more elaborate trigonometrical development. Since rapidity as well as accuracy is an object, we use a steel tape, fifty feet in length, which is fitted with a micronometer screw, adjustable to compensation for any possible change of temperature, and also with a dynamometric attachment to govern the tension applied. The measurements may be made on wooden plugs, which are driven into the ground along the alignment at intervals equal to the length of the tape. When all things are ready this line of two miles in length can be easily measured once over in one day. However, since the best tapes manufactured can be but inefficiently compensated for temperature, and since the best assistants are liable to a personal equation in sticking the marking pin, some invariably inserting it to the right of perpendicular and others the reverse, it is well that it should be measured several times and by different persons, and a mean of the results taken. Then it should be leveled, corrected for gradients, and finally reduced to the length of a concentric arc at the level of the sea, when it will be ready for use in the system of triangulation.

In the early stages of the development of this base, occurring on the level of the plain, it will be found necessary to use artificial signals. Great tripods of framework are erected and each of these is furnished at the summit with a flagstaff, to which voluminous folds of white muslin are nailed, while the body of the steeple is wrapped with the same material and decked with loose tatters and streamers, which, by their ceaseless flutter in the wind, offer occasionally a surface from which the light is reflected to the eye of the distant observer. These stations should be erected in conspicuous places, on high ground or the salient angles of bluffs, that the observer may know where to direct his instrument in searching for them, as it is extremely difficult to pick out the faint glint of a few yards of muslin on the broad white surface of a remote plain. As the development continues and climbs from the foot-hills into the high and peaked mountains, these natural points are sharp and distinct enough, and the labor of station building ceases, except in cases that are very unfavorable. True, this triangulation by natural points is not so precise as it is in our surveys of coasts, where even the phase of the conical signal is considered too important an element of error to be neglected, nor is it wise that it should be so, for a fault of a few feet, or even of a hundred feet, in the position of a mountain top located by this plan is of no practical consequence, and greater expenditure for greater accuracy is something which no people can afford. A mountain is only a land-mark by which travelers are assured of their place and are guided as they go, and to men who travel by land a fraction of a mile is a deviation which they cannot notice; to the voyager at sea, however, who may be wrecked at any moment, the exact site of the sunken rock which he shuns should be known to him, in order that he may certainly avoid it. This is why the coast and inland surveys are so different in the amount of cost and the degree of accuracy which characterize them.

It is now time for us to occupy our typical triangulation station. This may be a mountain 14,000 feet in height, whose summit is above all vegetation and in the belt of perpetual snow. It will be ascended by the engineer, the meteorologist, and such assistants as may be necessary to carry the implements of the work and the food and water for the maintenance of the party, and to build the stone monument which is to crown the mountain and receive the records deposited here. On account of the privation and exposure to which the corps is subject the time of occupation cannot well exceed one or two days, although, if possible, one night should be spent on the crest and devoted to the determination of the azimuth of some line radiating from here. Since the time is so short the observer should make all reasonable haste in his operations. Especially is this so in his sketches, over which he must not linger, which, if he is anything of an artist, he will be sorely tempted to do. He may see before him broader views and scenery more grand

and impressive than ever was painted yet, but picturesque effects are no business of his. To the topographer of artistic tastes there is great temptation to finish his sketch by inserting a pine-tree in the foreground, and, perhaps, an eagle's-nest in the tree; this is all very wrong, for such dalliance may cost the omission of that far distant peak, which is printed like a fine point against the horizon, and which, insignificant and low as it appears, is yet of vital importance to his scheme.

His sketch is perforce but the outline and skeleton of a picture. Two converging straight lines, with a stroke or two of shading hastily thrown in, are sufficient to represent the ordinary mountain peak. Yet, if this peak should possess any oddity or strong individuality of shape, this feature should be magnified in the drawing, and remembered as a key to the identification of this point when seen from elsewhere at some other time. In general the telescope is directed to the highest part of the mountain, which has been selected as a correlative station, because that is the spot which can be most easily identified, either from a distance or on the ground itself, but if this place should be uncertain, as where there are a number of pinnacles of equal altitude, or not sufficiently pronounced, as in a plateau summit, some peculiar object, as a lone tree or an isolated bowlder, should be chosen as a center upon which to sight. If the mountain is low and covered with timber it may be absolutely necessary to send axemen in advance to clear away all but the largest and most central of the trees of this forest. Since any mountain, when seen from different points of view, presents phases that are quite dissimilar, it is one of the greatest difficulties of triangulation to make sure of the identity of a station previously occupied, or, where there are a number of observers in the same field, to secure uniformity in the choice of the same.

The expert geographer is proficient not only in profile but in contour drawing, and on every mountain station he executes a contour plot of that scope of country which he sees beneath his feet, and of whose conformation he is certain. This completed, he reads angles for the direction of the spurs which project from here, and estimates with enough closeness such distances as may not exceed ten miles. But in the preparation of this local plot he should not be too comprehensive, and go beyond the bounds of certainty into the outer limits of conjecture. Every mountain is surrounded by valleys, on whose farther side are other ranges perhaps as high as this, and they form the limit beyond which no contour sketch should presume to go, else it becomes conjectural and unreliable. It may include those environs of valleys with a periphery of the foot-hills which are there *enceinte*, and a tracing of the cañons which indent the same, but no more. In the office a contour sketch is accepted as truthful evidence of the country as it really is, while a profile drawing is considered only a copy of the land as it appears to be, when uncorrected for the illusions of perspective, and is studied and deciphered accordingly. Looking abroad from this station, the successions of distant ranges, which are in reality separated by broad interspaces of valley and plain, are projected into a dense and circular wall, apparently unbroken by pass or intermission, whose serrated outline is seemingly as continuous as the horizon; it is an error to which the human eye and judgment is subject, and so, in orographic delineation, the impressions of the eye are to be received with caution, and only the readings of the theodolite are to be accepted in full faith.

The instrument of triangulation is a theodolite, whose accuracy and weight increase with the minuteness of its graduation, but, in this rough and laborious work, there soon comes a limit beyond which it is imperative to sacrifice nicety to portability. This is reached when the limit is graduated so as to discriminate to ten seconds of arc. With this the observer reads and repeats the angles, singly and in combinations, which lie between the visible points of the triangulation scheme, choosing for this important task the most opportune moments, usually in the evening or early morning, when the sun is behind the hills and the rim of the earth is seen in silhouette against the rosy background of the sky.

In addition he takes single readings to subordinate geographical features, which,

although they may never be occupied for the purpose of reciprocal observations, may yet be located by intersections from two or more triangulation stations. Sights are taken to the junctions of streams, the mouths of canons and to the heart of a distant village. Some point, or "tit," standing on the edge of an abrupt bluff, where the rapid descent begins, is used as a means of marking the end of a neighboring mountain range. A promontory, jutting into the confluence of two rivers, is instrumental in fixing the place of their union. A solitary butte on the plain, insignificant in itself, is very useful in determining the locus of the stream which flows by the side of it. A spot of green on the desert, evidence of a spring of water there, is located, for it will probably some day be camping-ground for him or his co-laborers. A minute patch of white lake-bed, or alkali flat, or red escarpment, is sighted upon, because on such a day he made a meander station there, and this sight will serve to check its position. In his note-book and mind he has dubbed all of these things with graphic titles, or designated them by letters of the alphabet, and by these tokens he will know them when he sees them again. But this system of names is only a transient device for the assistance of himself and those who work in concord with him, and must not appear upon the printed sheet, which is no place for the arbitrary nomenclature of any one man. Perhaps the modern geographer is guilty of no more common and high-handed outrage against right and beauty than by ignoring the appropriate and beautiful titles which abound in every country, however wild and uncivilized, and attaching his own, or, by mutual and tacit agreement, his comrades' names, to the mountains of that land, thus announcing themselves to the world as nostrums are advertised on the pyramids.

All of the preceding description which does not refer directly to the triangulation process is also pertinent to the topographical station, which may or may not be a point in a subordinate scheme of triangulation. Of course, it is desirable that every occupied station should subsequently be made an object of reciprocal observations and thus become a center of a plexus of triangles, the computation of whose sides will admit of the elimination of error and the distribution of spherical excess, and the observer should neglect no opportunity to confirm his position in this manner. Still the predominant idea of a topographical station is that it is a means of local topography. Angles are read to three or more known points, or triangulation stations, of which there are usually a number visible, which data, being plotted, are sufficient to fix the station in its proper place on the projected map. Then by lines of sight, which shall be intersected by other lines of sight from other topographical stations, the most prominent features within a radius of twenty or thirty miles are located, and, as a precaution, bearings are also taken to all eminent points at a greater distance, even to the horizon, as they may come into use in some future dilemma of map-drawing.

Thus it will be seen that every hill, however humble and inconspicuous, may be used as the site of a topographical station, provided there be three known points visible, and that the surrounding vista be not too narrow. A few hours are enough for its occupation, and the route between points of triangulation should be marked at regular intervals by the monuments of these stations. A topographical station in the streets of a settlement, or at the end of a mountain range, will locate these important places, and in camp, even in the center of a broad plain, there is no more profitable manner in which the topographer can spend the hour or two of leisure time after dinner, than by making a topographical station there and locating his position. Every camp thus fixed is a new initial point at which the meander of the next morning will begin.

The meander survey is useful in tracing the path of defiles and water-courses and the route of roads and trails, in determining the distances between springs of water, villages, valleys of pasture, fords of rivers, and other such information of interest to the future traveler, and finally as a commendable occupation for the engineer who is on his way from one mountain station to the next. In the theoretical journey of this kind, the engineer would follow the edge of the

dividing ridge from one peak to the second, from which lofty promenade he could see the earth like an extended scroll beneath his feet, and make a survey which would be exhaustive and complete. But in the real, hard practice, he finds this path an impracticable one, for it is broken by precipices and blocked by abutments a thousand feet in height. His easiest route of travel is by the side of flowing water, whose tendency it is to erode abrupt cliffs and soften steep gradients into an average and even slope. Besides, along the streams there are trails made by the wild animals which come here for drink and covert, and by the wild men who come hither to hunt and fish. Therefore, if the detour be not too great, the most expedient route from mountain to mountain is down one valley and up another, and the topographer, who traverses a valley without taking some sort of a survey of it, is almost criminally negligent of his duty. On the other hand, if in a block of mountains the preeminent peaks be occupied, and the streams which emanate therefrom be meandered, nothing more is needed for a most excellent topographical map of that country.

It is supposed that all travel and transportation of outfit, except in the ascent of high mountains, is accomplished on the backs of mules, which are the only animals that can endure through the vicissitudes of so hard a life. Riding in the saddle, the surveyor can devote but one hand to the grasp and protection of his instrument, the feet of whose tripod rest in a holster attached to the left stirrup. To facilitate his secure hold, the members of the tripod are thirds of a cylinder, which fold into the smallest possible compass, and are easily held in the grip of a hand. The instrumental part of the meander transit is neat, solid, and compactly constructed. Its graduated limb is of small diameter, and its horizontal vernier reads only to minutes, which is all very well, since no smaller divisions can be plotted on the map. This graduation is used in the occupation of topographical stations and at meander stations, where the view is extended enough to make it profitable to linger an hour or so in the accumulation of notes and sketches. Since the meander survey is from its very nature so hasty and loose, the system of frequent checks can alone make it valuable, and at intervals of every few miles, at every camp if possible, and especially at the crossing of divides and other eminences from which known points are visible, stations should be accurately located by the three-point problem. Each of these then becomes a new initial point, at which the survey begins afresh and the error again begins to accumulate.

The meander is affected by error of two kinds, of direction and of distance. The former is incurred in the survey of a tortuous valley, whose general course must be guessed, or in crossing a timbered country or pathless plain, where the surveyor is in a constant state of uncertainty as to whither he is to go, or, taking a back-sight, as to whence he has come. This error, it will be seen, is thrown by the law of chance alternately to the right and left of the true line, and so has a tendency in its elements towards mutual compensation, and in a measure it corrects itself. But not so the error of distance which is always plus and cumulatively so. The test of the odometer wheel, by which its number of revolutions per mile is ascertained, is made upon a level surface and along a staked alignment, giving a result almost absolutely correct. In practice, however, the vehicle climbs acclivities of every grade, tacks hither and thither as it follows the trail up the mountain, winds incessantly in its route through the forest, and is disturbed by frequent jolts and collisions along the rocky floor of the cañon. Hence an "overrun" in its record, which can only be remedied, and that approximately, by the judgment of the surveyor, who is taught by experience to estimate very closely the surplus in a given run, and who applies a deduction accordingly. To such perfection has the odometer survey been brought that it is no uncommon occurrence for a skilled worker to meander a closed circuit of a hundred miles, and, plotting the route, to find the plot also close within a fraction of a mile.

In the general traverse not the vernier plate, but the compass needle, on account of its greater convenience, is used, and nightly observations on Polaris, at or about its elongation, give the basis for computing the magnetic variation at

each camp, to be used in the reduction of the meander notes taken in the vicinity of that place. At every camp, also, the fickle aneroid barometers are compared with the cistern barometer, their errors are noted, and the vertical element of the survey starts from a new and true datum plane when the march is resumed. The zone of country included in a meander survey may extend to the farthest visible point, as a series of sights on a mountain even twenty-five miles distant will give its position to a close approximation, but its principal intent is the preparation of a narrow route map, the areas encompassed by whose windings will be filled in from the topographical stations.

From its nature and narrow scope, the meander survey is fuller and takes cognizance of objects more minute than can be noticed in the other systems, and in this the engineer is liable to a charge of partiality, reproved in the early part of this article. But this is not partially in one field at the cost of neglect in another, and the greater excellence of this work is so much clear gain. Moreover, since the meander is usually by way of roads of frequent travel, and since a map is useful, and should be excellent, exactly in proportion to the number of people who are guided by it, it is well that the meander plot should excel in completeness the representation of those almost inaccessible parts which will never be seen except by the hunter or bandit. On the same principle, in the United States geographical surveys, tributary to the War Department, whose maps are on a standard scale of eight miles to the inch, it has been deemed a matter of public economy to represent on one inch of space sixteen miles of some territory, as, for instance, on the level and vacant plains, while in other regions four miles to the inch will barely do the country justice, and in rare instances, as in rich and rugged mining districts, one inch of space is allotted to two miles of ground. In these surveys, which, under the direction of Lieutenant Wheeler, have attained to a most honorable organization and efficiency, material assistance is rendered by the executives of the working corps, who, as army officers, are skilled in the use of the sextant in practical astronomy, and take observations for latitude in the evening at camp. In geodetic surveys, however, the field astronomer is not so essential as he used to be in the old days of "straight-away" explorations and transcontinental surveys, which were too rapid to admit of being rectified by an accompanying belt of triangles.

Every transit, whether for meanders or triangulation, is fitted with a vertical circle, from which to read the angles of elevation and depression of those points which are located by intersections, in order to compute the heights of the same. These angles are recorded as plus or minus, according as the objective point is above or below the observer's station, whose altitude is invariably determined by barometric readings.

The frequency of meander stations in ordinary country will average perhaps one to the mile. In this as in the other departments of the survey too punctilious zeal will defeat its own intents by causing delay, and the surveyor who is too scrupulously exact in the forenoon will have to virtually abandon his task in the afternoon in order to reach camp by night. In a forced march, of twenty-five miles or more, the meteorologist and odometer man, the safe carriage of whose implements requires a slow and steady gait, may proceed at a walk after taking their readings at a meander station, which task will occupy them but a moment, while the surveyor lingers behind to make the necessary sketches and observations, and then, riding at gallop, overtakes his comrades before the next station is reached. Many such shifts as this are known to the practical and energetic topographer, who learns to emancipate himself from those pedantic and common-place rules that are found in books of surveying, and brings into play his powers of ingenuity and invention to adapt himself to the peculiar circumstances by which he is surrounded. If he finds himself alone, out on some trip of hasty reconnoissance, or on some hunting excursion on which he could not carry both rifle and transit, he draws from his watch-pocket an aneroid and from his saddle-bags a pocket-compass or an altazimuth, and his equipment for survey is complete; as for distances, he can estimate them, or determine them by the time they take, calculating at the

rate of three miles an hour, or, better still, by counting the steps of his mule, and allowing one thousand double paces for a mile.

These are the general divisions and some of the novel features of the geographers work in the field ; in the office it is not distinguished above the routine of office-work in general. This thing only may be noticed, that the hand to hand struggle which the field engineer constantly sustains with the forces and obstacles of nature blunts the acuteness of his prehensile powers and makes his touch too heavy for the fine drawing necessary in a map finished for publication, and there should be in every office a superior draughtsman whose hand is accustomed to the use of no heavier implement than the artist's pen. This artistic finish is bought by the sacrifice of accuracy, however, and between the field engineer and the final draughtsman there should be few, if any, middlemen to compile and replot the work, because only the man who has seen the country can produce its physical characteristics with truthfulness. In every copy that is subsequently made the face of the land grows more artificial and ideal, each mountain loses its individuality of shape and assumes a stereotyped and symmetrical regularity which it does not possess in nature, some of the niceties of truthful representation are magnified into exaggeration and others are overlooked and obliterated, the bed of every canon grows broader in each successive transcript, and the large hills grow larger as the little hills dwindle away. As in a popular parlor game a whispered story, passing current from mouth to ear throughout the round of a circle, grows strange and distorted beyond recognition, so in the successive reproductions, of a map by strange hands, it loses its photographic and pre-Raphaelistic truth of execution as the idiosyncrasies of the various draughtsmen are wrought into the plan, and it comes finally to represent a country Titanic and unnatural, made not by the accidents of nature but by the design of man, and moulded by the rules of a regular and rigid geometry.

OVERCOMING STEEP GRADIENTS ON RAILWAYS.

By Mr. HENRY HANDYSIDE.

Journal of the Iron and Steel Institute.

The chief object I had in view, when, in 1871, I first matured the idea of the new system, was to provide a cheap and efficient means whereby the locomotive of ordinary construction might be made available for surmounting steep inclines, and without any alteration or addition to the ordinary permanent way, or alteration in section of rails.

The subject is naturally divided into two parts ; the going up, and the coming down, and I propose to describe them in that order.

There is no portion of railway engineering so arbitrary and well defined as the law of inclines.

On all ordinary locomotive lines, the steepest portion becomes "the ruling grade" for the whole line ; that is to say, the ruling grade decides the weight of the locomotive, the exact load it can draw on that grade, and the weight of the rails along the whole length of that line, on which that locomotive has to run.

There is no difficulty in apportioning the steam power of any locomotive to the amount of its adhesion, and as this adhesion is solely dependent on the amount of weight which can be put upon the driving wheels of the engine, it follows that the load any engine can take up an incline must be in an exact *ratio* to the weight on the driving wheels, and the angle of the incline.

It has been ascertained, by actual experiment, that the limit of adhesion between an iron tyre and iron rail, is on an incline of 1 in 6 ; or in other words, any locomotive, with sufficient cylinder power, and all wheels motors, will ascend an incline of 1 in 6.

Any very close approach to this limit would be of little commercial value, and

the nearest which has been successfully employed, is an incline of 1 in 10, which was worked for three years on the Baltimore and Ohio Railway, the engine taking up a load as heavy as itself—this fact is recorded in Mr. Isaac's interesting paper, read before the Institute of Civil Engineers, on November 23rd, 1858.

Thus it appears evident that it is not the steam power of a locomotive that is wanting, but the adhesion between its wheels and the surface of the rails.

To supply this great want has been the object of locomotive engineers from the earliest days of the steam engine, and very numerous and varied have been the mechanical contrivances brought forward, many of them performing all requirements, but laboring under the disadvantage of additional cost to permanent way, greater weight and complication in the engine itself, and all being obliged to elevate their steam power to ascending the incline at the same time with the load.

It is on this point that my system differs from all others ever used. I use any ordinary locomotive, applying thereto a winding engine and steel wire rope, or chain, and peculiarly constructed gripping struts, which also perform the duty of a most powerful brake when descending.

The engine having hauled its train to the foot of an incline, of say 1 in 12, disconnects itself, but leaving the end of the wire rope fast to the train, it proceeds up the incline for any desired distance, but within the limits of the length of the rope.

The struts, having been released by the engine driver, immediately came into play, firmly grasping the heads of the rails, and thus the engine becomes at once a stationary winding-engine. By the application of steam to the winding cylinders the train is drawn up close to the engine.

If the incline is of too great a length to be surmounted in one lift, a similar pair of gripping struts are fitted to the last wagon or guard's van of the train, and as they act quite automatically, the train is firmly held in its place whenever the winding ceases.

This automatic action of the struts, when fully understood, will recommend itself for adoption in all cases where retrogade motion is to be feared on steep inclines; and even on those in this country on some of the main lines ranging from 1 in 40 to 1 in 60, and on which such disastrous results have followed from the breaking of couplings or draw bars.

In laying out a new line for my system, I prefer to keep the steep inclines straight, and within the limit of wire rope the engine can carry. I prefer not to exceed 300 yards, each incline to be followed by a piece of level, which may be taken advantage of for curves. Thus, the ascent of the line is made by a succession of steps, and resembling in its action the working of a canal with locks.

My chief reason for keeping the inclines straight, is to dispense with cast iron guide pulleys, which are objectionable, entailing great friction and wear and tear to the rope; if, however, the nature of the ground is such that the incline and curve must be combined, then the ordinary guide pulley may be resorted to.

When the inclines are kept straight but very few wooden rollers are sufficient, the rope bearing very lightly and only for a portion of the lift; this is due to the rope being coupled to the drawbar of the wagon and the top of the winding drum, at least 3 feet 6 inches from the level of the ground.

It is evident that by this transformation of an ordinary locomotive into a stationary winding engine, it combines and uses all the advantages to be derived from either or both.

Long experience, apart from the theory of the question, has determined the theory of the rope system, and the nearer the vertical lift is approached, the greater the economy of working—but the risk increases in equal proportion.

It is an easy matter to provide against an accident—when going up, either an incline or vertical lift—but the descent has to contend with the formidable powers of gravitation and accelerated momentum.

This brings me to the second part of my subject, "the coming down."

As I have mentioned, I prefer to cut up my line into steps—keeping each steep incline of a comparatively short length. By this means I can reduce the

danger to be anticipated from accelerated momentum; for, supposing the brakes to be overcome, which would never be the case until probably one-third, or one-half of a short incline had been descended, then all the speed any train could acquire sliding to the foot of the incline would soon be overcome when the train came on to the level.

We know by practice that all railway stock has an adhesion to the rail equal to one-fourth of its weight, although many engineers in this country do not think more than one-sixth ought to be relied on.

Taking even the latter as a datum for braking purposes, it is clear that any railway wagon or carriage, with proper brakes on all the wheels, could descend an incline of 1 in 12 with perfect safety, so long as a certain speed was not exceeded, but the great danger is that this speed might be exceeded, even under the charge of the most experienced brakesman, who would have no greater retarding power to apply to, and that train would "run wild."

Foreseeing that this evil must be provided for, I have so constructed my gripping strut that it acts as a brake of the most powerful nature when coming down hill.

The construction of this brake causes it to press, not only on the top of the rails, but also in as great a degree on the sides of the heads of the rails. To provide against wear, the three bearing surfaces of each shoe are made as renewable as pieces of iron or brake metal, which can be removed and replaced in less than 20 minutes.

This brake will work on any section of rail, but it must be apparent that the deeper and flatter the sides of the tops of the rails are the greater will be the effect produced with the least amount of wear on the renewable faces of the brake. Some who have seen the action of this brake, having at once admitted its great retarding power, have qualified the praise by saying it could not be generally available on our lines on account of "points and crossings."

This, at first sight, appears a most formidable objection, but, like every mechanical difficulty, it ought to be, and has been surmounted.

In the case of the application of my brake to a locomotive, Mr. Walker (of the firm of Fox, Walker & Co., Bristol) has made a very ingenious adaptation of steam power, by which the brake is instantaneously and automatically lifted off the rail when coming close to a point or crossing.

A similar arrangement, in the case of a guard's van, can be secured by the use of compressed air, or even by ordinary mechanical appliances.

Although I can fully agree with the general opinion that, "it is desirable that brakes should work equally well over points and crossings," I still think it will be admitted that, although danger is generally to be anticipated in the immediate vicinity of points and crossings that in nine cases out of ten, when brake power has been insufficient, the engines and trains were running on clear rails, and might have been brought to a stand before the points and crossings were reached, if the driver and guard had been in possession of some greater and reliable retarding power.

By the application of this brake to a locomotive, its retarding power is nearly trebled, an advantage of no small importance, especially when under the control of the driver, who is the sole and proper person to have full control over his engine and train.

All the retarding force which can be derived from the top surface of the rails has long been known and utilized to the utmost, as it has often been found insufficient on our ordinary lines, and as it would certainly be quite insufficient on steep inclines, I have ventured to utilize a portion of the ordinary rail which can well afford to take its share of work when required.

My great object in bringing the subject of overcoming steep gradients before your notice, is to prove that by this simple adaptation of certain well-known machines, in combination with various novel appliances, a railway system is produced which will enable the engineer to undertake the construction of mineral lines at a much lower rate than heretofore.

I do not say that the cost, mile for mile, will be less in all cases, as compared with a line laid out with ordinary grades; but it will enable the engineer to take a more direct route, and effect a

large saving in actual distance or length of permanent way—generally as much as 60 per cent., and after giving him the power of taking his line in certain directions to suit the wishes of land-holders, in some cases, thus removing the chances of a strong opposition, which, in several instances, has prevented an easy access to districts, known to be rich in mineral wealth.

BEST TYPES OF WAR VESSELS.

From "Iron."

A HIGH compliment was paid by Admiral Sir R. Spencer Robinson to the "Naval Prize Essay, 1876, on the Best Types of War Vessels for the British Navy" when he occupied five times the ten minutes allotted for speeches at the Royal United Service Institution in defending himself and his late administration from the supposed aspersions of the essayist. Commander Gerard Noel, R N., the successful essayist, is a young officer of much professional promise, who is not unknown to fame. He is not a naval architect, but a seaman; and those who think seamanship ought to be allied to ignorance, should not be shocked if he has fallen into serious errors of detail. The general principles stated by the essayist are at least clearly laid down, and if they had not been worthy of discussion we cannot suppose that Sir Spencer Robinson, Mr. Barnaby, the Controller of the Navy, and so many naval constructors would have assembled to discuss the prize essay.

If Sir Spencer Robinson's argument proved anything, it was that the *Vanguard*, now at the bottom of the Irish Sea, was so perfect a type of war vessel that no improvement, at least from outsiders, was possible. This doctrine of finality is one of the last which Admiral Robinson should uphold. His own practice, and the practice of his successors at the Admiralty, has ever been the reverse of finality. Every ironclad ship built since the *Vanguard* and her sisters, has been not only different from that vessel, but also from its own immediate predecessor. Nobody, indeed, dreams of reproducing a *Vanguard*, or, indeed, any existing ironclad. Mr. Barnaby himself advocated, three years ago, a type of ironclad substantially the same as that suggested by Commander Noel. In fact, the *Nelson* and *Northampton*, now being built, were probably in the essayist's mind when indicating the best type of ironclad. Moreover, Mr Reed, who has never been wedded to finality, in advocating a modification of circular ironclads, shows himself open to a change of type.

Another fallacy relied upon by Sir Spencer Robinson is that a ship of war built for a special object cannot be said to have failed, if she fulfills that object. The whole question is as to the reasonableness and desirability of the object sought. For example, the *Vanguard* (his favorite ship) is built with an exceptionally thin bottom to carry a heavy patch of armor on her topsides; but her bottom ought to have been devised for colliding, inasmuch as she was armed with a ram for the purposes of hostile collision. Yet Sir Spencer Robinson says that the *Vanguard*, at the bottom of the Irish Sea, has fulfilled the objects for which she was designed, and ought not to be criticised by an officer so young in years as Commander Noel.

But the favorite fallacy of Sir Spencer Robinson is that a ship of war is designed solely to encounter a foreign ship of her own special type. This fallacy runs through the whole armaments, as to guns, rams and torpedoes, of the British fleet. And the gallant admiral illustrated this monstrous doctrine by the case of the *Nelson*, our latest broadside ironclad. The water-line of the *Nelson* is protected by 9-inch armor, which also extends upwards at the ends of the battery, so as to protect the crews from raking fire; but the battery is unprotected on the broadsides. The object contemplated by this arrangement is to give thicker armor over the vitals of the ship at the expense of the gunners, and to increase the offensive power of the ship by carrying a heavier weight of ordnance.

Now, Sir Spencer says, in so many words, that the *Nelson* is a splendid ship, but is not intended to fight ironclads whose ordnance is protected by broadside armor. In other words, every British ship is to run away from every hostile vessel which is not precisely of the same type as herself. Sir Spencer forgets that the choice of fighting or running away may not always lie with the British ship. And, can it be for a moment admitted that the *Nelson*, being unable to run away fast enough from a more thickly armored Japanese, South American, or even Turkish ironclad, should strike her colors without trying the fortune of war? When Commodore Nelson, in the third-rate ship of the line, *Captain*, at St. Vincent, singled out a Spanish three-decker of the first rate for close action, he set an example which England will ever expect the captain of the ironclad ship *Nelson* to follow whenever he may be called upon to do so.

Commander Noel's strong point is the sinkability of ironclads, and with this Sir Spencer did not grapple; yet it is vital in the case of vessels built for collision. It belongs to the very nature of iron, as a material for the skins of ships, that it should be much more easily and more fatally perforated than wood of corresponding strength. Thick patches of armor, forming semi-invulnerable targets, protect parts of the ship from shot, but a poke from the smallest gunboat furnished with a snout would send the best ironclad to the bottom. Why, then, are not all the unarmored ships provided with stems shaped for ramming? As it is, British unarmored sloops and corvettes have no choice but to run away from a hostile ironclad; and if their speed be insufficient for successful flight, they can not avail themselves of such chances of war as remain to them, because their stems are not armed for perforating the thin bottoms of iron ships, their guns are not adapted to penetrating the armored topsides, and they are unprovided with torpedoes. The teeth of the British Lion are drawn, under the idea that he is only to encounter antagonists with hides of similar thickness. Naval encounters occur under countless varieties of conditions, but the chances are very many against special British types of ships ever meeting in action precisely the same classes of vessels. The occasions will be rare in which future Sir Philip Brokes will send written challenges to hostile *Chesapeakes* of similar scantling or armor.

On the one hand, every British ship, of whatever size, ought to have teeth that will bite any hostile vessel from which she cannot run away. And on the other, the bottoms of English ironclads need to be made secure against every puny antagonist.

The Chairman of the Royal United Service Institution has himself had some experience of what a small Irish passenger steamer, proceeding at low speed, can do to endanger the safety of an ironclad. Had that steamer been going at high speed, in rough water in the open sea, Sir Henry Codrington might have had to report the loss of the ironclad *Hotspur*. Some very awkward scratches have been received by other ironclads, the *Northumberland*, the *Defence* and the *Warrior*, for example; which have long shown that ships intended for hostile collision, and specially open to being rammed by much smaller and inferior vessels, cannot collide with even a tug-boat without serious danger.

Mr. Barnaby endeavored to face this point by showing that the new *Inflexible* is to be provided with nine horizontal safety tanks, four feet deep, at the water-line. But these tanks give no more safety than a water-line armor belt. They are simply a target to be avoided by artillerists. A blow below the belt would sink the *Inflexible* as easily as the *Vanguard*, whereas a shot through the upper works may place a few men *hors de combat*. A tug-boat furnished with a submerged prow might send her to the bottom. This circumstance points to an advantage which all small craft ought to be prepared to seize if forced into unequal encounter; and at the same time it points to a great element of weakness to be guarded against.

In short, attention has been so concentrated on the armor-carrying power of ships that their bottoms have been much neglected, both defensively and offensively. We do not think Commander Noel's suggestions as to the cure go far enough. Torpedoes defensively employed ought to be seriously studied with a view to deterring the ramming attacks

of inferior vessels. The towing torpedo lends itself very readily to such defensive employment. And the chances of war between a short, handy English sloop, forced by circumstances to contend against a foreign *Inflexible* and her four guns, though desperate, would not be so hopeless, if the sloop had a projecting prow, that a British commander could be justified in striking his flag before attempting to strike a blow.

Commander Noel is to be the more commended for his essay, that he has fearlessly expressed opinions which officers of higher rank have long held, but which they could not safely express. The gag is so enforced by the system of promotion and the terrors of retirement that few naval officers will run the risk of publicly expressing any opinions on professional subjects. Differing from his conclusions widely on many points, we conceive highly of his method of approaching the subject. And, in any case, an essay which the whole constructional staff of the Admiralty thinks it important to refute in open discussion, cannot be without merit.

The prize has been adjudged by three of the most eminent admirals, and, much as we differ from many of its details, the essay has in it much which calls for serious consideration and high commendation.

ON NEW DETERMINATIONS OF THE VELOCITY OF LIGHT.

Proceedings of the Royal Society.

The old philosophers and astronomers, until Galileo, thought that the propagation of light was instantaneous.

Astronomical Determination of the Velocity of Light.—Rœmer, a Danish astronomer, called to the Paris Observatory by the illustrious Picard, after having computed from some old observations the eclipse times of Jupiter's satellites, found great discrepancies between the calculated and observed times; the eclipses appear too soon when Jupiter approaches the earth and too late when it goes away. Rœmer ascribed these differences to the time necessary for the propagation of light, and concluded from his observations that light requires about eight minutes to come from the sun to the earth.

Bradley, one of the most illustrious English astronomers, seeking to put in evidence certain small annual motions of the stars caused by the displacement of the earth in space (annual parallax), found such a motion, but quite different from the expected one. The apparent deflection of the direction of a star—for instance, γ Draconis, near the pole of the ecliptic—instead of being at every moment directed, as expected, towards the centre of the terrestrial orbit (the sun), is directed at a right angle. The greatest elongation (called *aberration*) rises to $40.7''$ from six to six months. Bradley, after many attempts, ascribed this effect to the composition of the velocity of light with the velocity of the elliptic motion of the earth (1728).

From those observations, and from the approximate knowledge of the distance from the sun to the earth, the velocity of light was found equal to about 200,000 *English miles in a second*, in other terms, *one million times the velocity of sound.*

Direct Determinations of the Velocity of Light were for a long time considered as impossible, owing to the enormous value of this velocity. The first solution was given by M. Fizeau (1849) by the method of the *toothed wheel.*

Induced by some considerations analogous to the celebrated *acces theory* of Newton, M. Fizeau, one of the most illustrious members of the Paris Academy, and recently elected Honorary Member of the Royal Society, succeeded in rendering perceptible and even measurable the duration of the propagation of light for a distance of a few miles.

The principle of the method is the following:

A beam of light passes through the interval between two teeth of a rotative toothed wheel (*roue dentée*): this beam is reflected on a mirror fixed some miles distant, comes back exactly on the same

line, and passes again through the same interval as before. An observer can receive this beam: he will see a luminous point, a *luminous echo*, through each hollow between two teeth: if the wheel revolves with an increasing speed the luminous impression will first become continuous. The wheel will soon revolve with sufficient rapidity to turn a small angle during the time necessary for the beam of light to go and come back again. The angular velocity can be so regulated that the solid part of a tooth is substituted for the hollow part during this time; then, on coming back, the beam will be obstructed by the wheel. The same obstruction will take place at each tooth, and the luminous echo will disappear.

If the velocity of the wheel be doubled, the luminous point will appear again, because the reflected beams will meet with the following hollow and pass through. With a triple velocity a new extinction will take place, as before.

The following apparatus is necessary to produce the exact reflection of the beam:—At each station a telescope is directed to aim at the centre of the object-glass of the opposite station. The beam of light is sent through the first telescope: the pencil of rays, rendered nearly parallel, is received by the second and concentrated in its focus, and there reflected by a small mirror. After reflection, the rays follow exactly the same path, and come back at the very point they start from. The observer can receive these *return rays* without being blinded by the source of light, by interposition of a piece of transparent glass, which reflects a good part of these *return rays*.

M. Fizeau's Experiment (between Suresne and Montmarte) was made to prove that it was possible, not only to establish the duration of the propagation of light, but also to measure its velocity without the intervention of astronomical phenomena. The distance of the stations was 8633^m, about 5½ English miles. The number found by M. Fizeau agreed sufficiently with the astronomical result to give the greatest confidence in the exactness of the method, when applied under fair conditions. A new experiment was arranged with Arago in the Paris Observatory, but Arago's death prevented the execution of this design.

Professor Cornu's Researches.—First experiments were made between the Polytechnic School in Paris and Mont Valerein. (Distance, 10310^m, about 6½ English miles.)

His researches were conducted with a view to improve the method of the toothed wheel, in order to obtain the greatest exactness. The chief difficulty for the practical application of this method is to measure the angular motion or velocity of the wheel, to which the velocity of light is directly compared. The simplest means would have been, as in M. Fizeau's experiment, to give an uniform motion to the wheel; but such a motion is practically impossible to obtain, so it was necessary to find another mode of measure. The principle of the new improvement was the use of an *electrical registering apparatus*, to register the continuous increase of motion of the wheel. With that arrangement an exact uniform motion is no longer necessary, the observer being able by a peculiar electric signal to point out the instant at which the right velocity is obtained.

The second improvement, and one very important for the exactness of the method, is the substitution of a pair of observations of the return rays, when reduced to a determined feeble intensity, for the single observation of a total extinction.

These improvements, experimentally tried in 1872, gave the velocity of light as 298,000 kilometres per second. The probable error does not rise to 1 per cent.

Professor Cornu's New Determination was made between the Paris Observatory and the tower of Montlhéry. (Distance, 22910^m, about 14¼ English miles.)

A direct determination of the velocity of light was ordered at the beginning of 1874 by the Council of the Paris Observatory, on the proposal of M. Le Verrier, Director, and of M. Fizeau, Councillor. The best conditions were chosen for the optical and mechanical apparatus, and the stations were placed at an increased distance. One was erected upon the higher terrace of the Observatory, and supplied with a telescope of 0.38^m (1¼ foot) aperture, and 9^m (30 feet) focal length. The telescope and the remainder of the

apparatus (toothed wheel, registering cylinder, clocks, &c.) were sheltered under a large cabin constructed on purpose. The opposite station was erected on the top of the tower of Montlhéry; it contains only a reflection telescope sheltered by a cast-iron tube.

The experiments were made in the summer of 1874. The average of 508 pairs of observations gave the velocity as 300,400 *kilometres in a second of mean time.* The probable error appears not to exceed one-thousandth.

Second Solution for Direct Measurement of the Velocity of Light was obtained by the method of the revolving mirror (1850; based on the use of it by Sir Charles Wheatstone in his beautiful researches on the Velocity of Electricity (1834). Arago, after an enthusiastic account of these researches before the Paris Academy, showed how the new apparatus might be adapted to solve some most important problems of optics (1838), and specially to decide between the emission and the undulatory theory of light. He gave (April, 1850) a full description of his own attempts on the subject, but he was not able, through failing eyesight, to fulfill his design. Some days after, the complete solution of the problem was brought before the Academy simultaneously by Foucault and by MM. Fizeau and Bréguet. Foucault, in the year 1865, improved in several points the revolving mirror method, and obtained a direct determination of the velocity of light (298,000 kilometres.)

The principle of the experiment is as follows:

A beam of light reflected on a revolving mirror is normally reflected by a fixed concave mirror, and comes back again on the evolving one: during the time of the propagation of light from the first mirror to the second and back, the revolving mirror has suffered a little angular motion; the new reflection on it produces a small deflection on the return beam; from that deflection the velocity of light can be computed.

This method is certainly one of the most curious, but the deflections are so small and the march of the rays takes place in such extraordinary circumstances that it is difficult to ascertain the degree of approximation of the result.

Physical Importance of the Direct Determination of the Velocity of Light.—The importance of the result is perhaps greater for those physicists who occupy themselves with electricity than for those who work on optics. The beautiful experiments and theories of Prof. Maxwell, Sir William Thomson, &c., so clearly expounded by the British Association Electrical Standard Committee, have shown that the velocity of light is a coefficient common to the undulatory waves, and to the mode of motion which is called electricity. Several determinations, but purely electrical ones, have been made in England of that coefficient, and the results agree as well as possible, in that delicate matter, with the above given value.

Astronomical Importance of the same Determination.—The numbers measuring the phenomena discovered by Rœmer and Bradley, combined with the approached distance of the sun to the earth, have a hundred and fifty years ago furnished an approximate value of the velocity of light. Now the progress of science requires an inverse march; the exact value of the velocity of light permits, by the inverted calculus, the computation of the mean distance of the sun or the sun's parallax, that is to say, the same element which is directly given by the transit of Venus. Professor Cornu's last result, combined with Delambre's equation of light (deduced from more than a thousand observations of eclipses of Jupiter's satellites) or of Bradley's aberration value, which seems one of the best determined number, agree exactly with the result obtained by M. Le Verrier in his researches on planetary perturbations, and with the already known results of the last transit of Venus observations.

THE South Australian Government have contracted with Messrs. W. Simons & Co., Renfrew, for one of their patent hopper dredgers, which will be the largest of that type yet constructed, as it will carry 1,000 tons of its own spoil, and dredge to 30 feet depth of water, and steam out to Australia. This vessel will be built under the direction of Mr. Kinipple, consulting engineer of the harbors of Greenock, and also of the firm of Kinipple & Morries, of Westminster.

THE USES OF FERRO-MANGANESE.

By M. F. GAUTIER, of Paris.

Journal of the Iron and Steel Institute.

The use of specular pig iron, or spigeleisen, as it is termed in Germany, has been a great success in the Bessemer process. For some time after spiegeleisen was first employed, its addition was merely regarded as a re-carbonizing agent, which was rendered necessary on account of the protracted refining—the required hardness was obtained by the admixture of a pure and highly carburetted pig iron, without taking into consideration the part which manganese might play in the operation. Thus steel manufacturers selected their spiegel iron on account of its good and more or less foliated appearance, as this description of pig iron was alone capable of imparting to steel that body which had been eliminated by the protracted blowing. Notwithstanding that there were other pig irons which contained a sufficiency of carbon, it was at length found that manganese was the agent contained in the spiegel, which answered the purpose required.

In 1866, M. Valton, who then conducted the steel works at Terre-Noire, expressed his views relative to the reducing properties of manganese, in the *Bulletin de la Société de l'Industrie Minerale de St. Etienne.* He was of opinion that the iron oxyde, with which the Bessemer metal was saturated at the end of the process, was magnetic oxyde, and not peroxyde. He concluded that this was the case, because of the high temperature employed.

The magnetic oxyde, having only the same affinity to silicon as peroxyde, does not come away with the slag, but remains in the pig iron, and thus makes it red-short and unfit for rolling. If, however, we add, in the form of spiegel, metallic manganese—a body which takes up oxygen more readily than iron—the oxyde of iron is converted from the steel bath into a state of protoxyde, and the silicon takes this up to form slag. The reaction is represented by $Mn + Fe^3 O^4 = 3 Fe O + Mn O$; and the manganese is also combined with the slag.

Recent improvements in the method of ascertaining the proportion of manganese in iron—a chemical problem which at one time could not be solved—have shown that, in the Bessemer or Siemens-Martin metal, there is always, after the introduction of the spiegel, a small quantity of manganese, which sometimes amounts to half that introduced. Is it to be concluded, therefore, that the manganese remaining in the metal is beneficial—that it forms with the iron an alloy which improves the quality of the steel? Again, is it necessary, in order to introduce sufficient manganese into the metal, that an excess shall be introduced? Recent experience has shown that manganese, without doubt, acts as a reducing agent in the oxyde of iron. M. Bender, the engineer at Krupp's Steel Works, at Essen, has ascertained that 0.35 per cent. of oxygen is sometimes found in Bessemer metal before the introduction of spiegel. It may be taken for granted that the addition of manganese imparts to steel new and useful properties, and when it was discovered that this manganese was the active element of spiegel, it is not surprising that experiments were soon undertaken with a view of condensing it in a special product. The result of the experiments which were practically conducted by Mr. Henderson, of Glasgow, was that ferro-manganese was introduced into the market. The yield of manganese did not exceed 25 per cent., and its characteristic feature was that it was not affected by the magnet. When, in England, the manufacture of the alloy was given up, the Terre-Noire Steel Works Company purchased the patent right from the inventor, perfected the process, raised the yield of manganese to 75 per cent., and reduced the price 50 per cent.

The object of this paper is to consider the principal uses to which this alloy of iron, manganese, and carbon—ferro-manganese by name—is applied.

1st. MANUFACTURE OF SOFT STEEL.

Those manufacturers, who are special-

ly anxious about the quality of their produce, have always used, in the final addition, in the Bessemer or Siemens-Martin process, 1 per cent. of manganese, in the proportion of 10 per cent. of manganese. The spiegel contained 5 per cent. of carbon, and therefore the addition of 1 per cent. of manganese always increased the carbon to 0.5 per cent., and it was difficult, if not impossible, to produce softer steel.

Mr. Henry Bessemer was the first to discover that, in introducing a small quantity of ferro-manganese, sufficient manganese for the reduction could be obtained, and the quantity of carbon would be considerably lessened also. As it is not necessary to melt the ferro-manganese, or to deal with the refuse that is occasioned by the smelting of spiegel, the quantity of manganese added might be less than 1 per cent. if the yield of carbon were diminished, but it is found preferable to have 1 per cent. as the standard. It has been shown that 1 per cent. of manganese in the form of 10 per cent. of spiegel, introduced 0 5 per cent. of carbon into the metal; and 1 per cent. of manganese, in the form of 2 per cent. of ferro-manganese at 50 per cent., will require not more than 0.1 per cent. of carbon to be added. Experiments have shown that the richer the reducing alloy is in manganese the less carbon remains in the produce. Ferro-manganese containing 75 per cent. of manganese is, therefore, the best where the softest cast metal is required.

The following is a comparative statement of the average resistance of steel, manufactured from the same quality of pig iron, but in the first instance spiegel was added, and in the second ferro-manganese :

	With Spiegel.	With Ferro-Manganese.
Limit of elasticity	22 tons per square inch	16 tons per square inch.
Breaking strain	38 tons per square inch	28 tons per square inch.
Elongation per cent. measured over 8 in.	8 tons per square inch	25 tons per square inch.

This decrease of the breaking strain, with the increased elongation, is a decided advantage where hardness of the material is not specially required. The metal, which withstands a heavy breakage load with a small final elongation, has a special elasticity in the shape of resistance to change or form, which is apparent even when it is worked hot. In irregular pieces, this causes tensions, which often induce breakages when cooling, and it was to this unstable equilibrium of the molecule in hard steel, that we must attribute, for the most part, the inapplicability of steel to anything but rails and tyres. It is necessary, when steel is to be used for plates, forgings, machinery, and such like purposes, that it should be very soft, and for these purposes consumers should not require it to stand a heavy breakage strain. In practice, the loads are always supposed to fall with greater or less velocity, and are something similar to a shock. From the construction point of view, the exact value of a material is the product obtained by multiplying the breakage strain by the final stretching, and not the breakage strains alone. These views were long since promulgated by Mr. Mallet, and are well known to English engineers. Applying them to the two kinds of steel named above, and also to common iron, we find :

Hard ordinary steel	305
Soft steel	700
Common iron	105

Experiments made in France recently, have shown that a cannon ball projected against a wall, stayed by soft steel T bars, caused only one-third of the damage that resulted when common T iron stays were used. If hard steel had been employed, the result would not have been the same as when common iron was used. These experiments afforded proof of the superiority of soft material in resisting shocks. Moreover, the limit of the elasticity of soft steel, as compared with that of iron—16 to 9—must be a sufficient guarantee against small imperfections in structure. It is even in the proportion of 9 to 16 that it is advisable in a general way to employ soft steel, when the diminution of dimensions which results therefrom does not cause practical difficulties. A saving in the weight of material, sometimes amounting to 40 per cent., will result, and, in most instances, this will mean decreased expenses, because the price of common iron

and steel, in the future, will not show such a difference as they do now, steel in general being only 25 per cent. dearer than iron. The superiority of soft steel is still more evident, if we consider its use in comparison with that of the best classes of iron, such as those of Low Moor, Bowling, &c. At the Crewe Works, under the direction of Mr. Webb, the advantages of ferro-manganese have been made apparent in the manufacture of certain pieces of machinery, which require great care in their production. The unlimited development of soft steel is only a question of time. The output is now equal to the demand, but as consumers increase their consumption of the material, the production must be increased also. Its use, in the future, will become a necessity, as the substitution of steel for iron rails progresses. In France, and in England, owing to the recommendation of Mr. Barnaby soft steel plates are used in a general way in the Government shipbuilding yards, and the result is that the hulls weigh less, and more room is left for the cargo or the storage of coals.

The best way to adopt in employing ferro-manganese is to bring it to a red heat in order to facilitate the chemical reaction, and to avoid the scintillation which is produced when a cold body comes in contact with the liquid steel. This heating is readily done in the Siemens-Martin process by means of the furnace used for heating the materials of the charge. In the Bessemer process the ferro-manganese may be heated by replacing it—as the weight is less, the alloy being richer—in an iron vessel, suspended in front of the mouth of the converter. When the heating is completed the vessel is emptied into the converter, which is overturned at the same time. The reaction is perfect, and the running of the metal into the moulds mixes the two completely.

Comparing now the use of ferro-manganese with other methods which have been adopted to produce soft steel, it may be stated that the general idea was to restrict to the smallest possible limits the addition of spiegel, so as to lessen at the same time the proportion of carbon. This, however, produced a red-short steel, which was scarcely fit for rolling. In carrying on the Bessemer process, it is found impossible to go beyond a certain point in preventing an excess of iron oxyde in the mass of molten metal. On adding the spiegel, a violent reaction takes place between part of the carbon in the spiegel and the dissolved iron oxyde. The result is an instantaneous production of carbonic oxyde, which often forces part of the contents out of the converter. Of course, the metal is softer, but the process is uncertain, very costly, and dangerous to the men. Lastly, when the pig iron intended for the Bessemer process contains 3 per cent. to 4 per cent. of manganese, a steel fit for rolling is obtained without the final addition of spiegel, and therefore, a soft steel is produced. The oxyde of iron reacts upon all other oxydizable materials —first upon the silicon, then on the carbon, and lastly on the manganese. If, after the expulsion of the silicon and carbon, sufficient manganese is left to reduce to protoxyde all the magnetic iron oxyde which is produced, the metal is not red-short, as protoxyde of iron and protoxyde of manganese combine with the slag. Thus a malleable product, free from carbon, is obtained without the necessity of adding spiegel. This process, however, is both a difficult and uncertain one, for where it is adopted at all, it is necessary, in order not to spoil the manufacture, that the 3 per cent. to 4 per cent. of manganese required shall exist naturally in the pig iron. Where such pig iron is used, great skill is required on the part of the operator, as the spectroscope is not of any use, and the color of the slag is the only guide which can be followed. From time to time the "blow" is stopped, and by means of an iron bar samples of iron are taken out. When the quality of the product is found satisfactory, the process is stopped, but it requires great skill to tell when this point has been reached. Generally the product is not regular, and owing to the high temperature employed it still retains, in spite of its little carbon, a particular crystalline structure, which is found also in Bessemer operations, where the pig iron is saturated with too much silicon. It is certain that in countries like Austria, and the part of Germany where this process is used, soft metal, owing to the nature of the pig iron, is not met with

as a merchantable produce, and has not thus far produced good results. Ferro-manganese is there of great advantage, and to employ it usefully, the manufacturers are doing all they can to lessen the yield of manganese in pig iron. Ferro-manganese, therefore, is more generally being brought into use, as it is found to be the means of obtaining soft steel, and the reduction in the price is assisting in increasing its use, seeing that the employment is rendered more economical and judicious. Another advantage is, that it can be applied as well to the Bessemer as to the Siemens-Martin process.

2d. USE OF INFERIOR MATERIALS IN THE MANUFACTURE OF STEEL.

The most important properties required in metals produced from iron are fusibility, hot forging or rolling, and various degrees of toughness. The chief property of the carbon is to impart fusibility to the iron, but, at the same time, it lessens and sometimes altogether destroys its rolling or forging qualities. Pig iron thus derives its quality of being fusible and not rolling to the great proportion of carbon it contains. When the yield of carbon is decreased, the fusibility is also lessened, but the property of drawing out is increased. Carbon, therefore, destroys the quality of drawing out.

Experience has proved that in producing a smelted metal nearly void of carbon, metal similar to the softest qualities which have been spoken of above, it may contain up to 0.5 per cent. of phosphorus, 0.5 per cent. of sulphur, one per cent. of silicon, and from two to three per cent. of manganese, without losing its drawing out properties. An alloy of silicon and iron without carbon, but with 7½ per cent. silicon, has been perfectly forged. The question may be asked, whether the carbon acts in opposition to these different bodies, or is it the carbon only which exercises the unfavorable effect in the drawing out. Two causes seem to be at work, but it is difficult to explain the phenomenon. Whatever it may be, metallurgists have changed their opinions considerably on this point, and no doubt the future will decide the question, and iron will be presented under a new form to the trade. It is like a productive mine of new alloys suddenly put at the disposal of mankind, which must make its influence felt.

We shall not speak here of the silicon, as it is not found to any great extent in steel, except when it is afterwards purposely introduced. Sulphur and phosphorus, on the other hand, are often found in the production of steel, and it may be taken for granted that the former can be got rid of completely, if the quantity does not exceed the already considerable amount of 1 per cent. This is well known; it is sufficient to work a blast furnace with calcareous slag, and to add, if necessary, oxyde of manganese. The real enemy of the steel manufacturer, that which is met with everywhere, and is found regularly in all the successive operations, in the phosphorus. This is the most serious impurity, and is the most difficult to deal with. Before the effects of the various foreign bodies in steel were generally appreciated, it was taken for granted that with more than 0.05 per cent. of phosphorus, it was impossible to make anything fit for ordinary use with a yield of 0.5 per cent. of carbon, which steel then possessed, it was in fact difficult to exceed that limit without incurring serious losses. The rolling was the insurmountable obstacle as far as the phosphorus was concerned. The difficulty, however, was, to some extent, overcome by hammering the ingots, and undoubtedly this expensive and not very sensible operation (which some antiquated people, principally in Russia, still persist in maintaining in their specifications) has never been of any more use than to facilitate the absorption of somewhat unimportant matters. At present, it may be said that steel for rail making may contain up to 0.5 per cent. of phosphorus, and still be fit for rolling perfectly, that is provided traces only of carbon are contained in it. It has been long known that phosphorus made the rolling of iron easy, whilst phosphorus and carburetted steel could not be rolled.

It must, therefore, be concluded that phosphorus steel, which was not curburetted, would regain this facility for rolling. It was necessary that the question of making soft steel should be solved in a practical and certain manner, and this discovery was, therefore, dependent upon that of ferro-manganese establishing a new use for this alloy. Upon this subject I will now speak.

BENDING TESTS.

BESSEMER STEEL AND PHOSPHORUS STEEL RAILS.

Nord, France, 61 lbs. per yard.
Distance apart of bearings, one metre (3 ft. 3⅜ in.)

AVERAGE OF 25 CHARGES.

Loads in lbs.	Bessemer Steel.		Phosphorus Steel.		Description.
	Inches.		Inches.		
27,600 ..	0.1	0.0 ..	0.09	0.0 ..	Bessemer steel.
38,600 ..	0.12	0.0 ..	0.11	0.0 ..	Carbon 0.45 to 0.55.
44,000 ..	0.14	0.0 ..	0.13	0.0 ..	Manganese 0.15 to 0.25.
55,200 ..	0.17	0.08 ..	0.17	0.04 ..	Phosphorus 0.04.
66,500 ..	0.32	0.12 ..	0.94	0.73 ..	Phosphorus steel.
72,500 ..	0.64	0.40 ..	1.80	1.24 ..	Carbon 0.15 to 0.20. Manganese 0.25 to 0.35.
Breaking Weights.	102,000 to 110,000	..	97,000 to 108,000	..	Phosphorus 0.27 to 0.32.

The bending tests show that up to the point of elasticity, phosphorus steel behaves in much the same manner as Bessemer steel of ordinary purity. Further on, the permanent pitch is stronger, the metal gives way more under pressure, and the breakage takes place somewhat sooner than with pure steel. These results are confirmed by the tensile tests made with the same materials, particulars of which are given herewith :

TENSILE STRENGTH.

The Rails are those of the 25 Charges above.	Tons per Square Inch. Limits of Elasticity. Tons.	Breaking Weight. Tons.	Elongation per Cent. Over 8 in.	Over 4 in.
BESSEMER STEEL.				
Samples taken in the head of the rails.....	24.7 ..	49.7 ..	7.0 ..	8.5
Samples taken in the flanges.....	26.0 ..	48.1 ..	7.0 ..	8.5
Samples rolled into plates.....	23.8 ..	45.6 ..	9.5 ..	11.2
PHOSPHORUS STEEL.				
Samples taken in the head of the rails.....	24.2 ..	33.2 ..	9.5 ..	11.2
Samples taken in the flanges.....	25.5 ..	35.5 ..	10.2 ..	12.7
Samples rolled into plate	21.3 ..	36.5 ..	17.7 ..	21.3

The rolling into plates ⅜-inch does not much alter the hardness of ordinary steel, but it softens phosphorus steel to a remarkable degree. It is possible that part of the carbon is consumed by this thin rolling, but it is more likely that there is a new fact brought to light—viz., the destruction of the crystalline state of the metal through the alteration and compression of the matter. The fracture which, at the beginning was granular, has a tendency to become foliated. This is a very curious molecular modification, the most important practical result of which is to give to the metal power of resistance to shocks. Starting with an ingot of heavy section, and rolling the rails in two or three lengths, more phosphorus may be admitted without altering its power of resistance ; and, on the contrary, with an equal yield of phosphorus, a greater resistance to shocks is imparted to the rail. This fact has nothing contrary to rule, and has already been proved by the fact that phosphorus iron has been rolled into small bars, and this is well known by the ironmasters of Cleveland and the Moselle department. The resistance of phosphorus rails to shocks varies with the severity of the tests. In France, where the resistance to shocks is measured only by a ram of 300 kilogrammes (662.06 lbs.) falling from a height of two metres (6 ft. 6¾ in.), phosphorus steel stands the tests perfectly well.

Falling (shock) test on the preceding rails, ram of 300 kilogs. (662.06 lbs.) ; anvil, 10 tons ; distance of bearings apart, 1.10 m. (3 ft. 6⅜ in.) ; rails of 61 lbs. per yard.

When the rails have to stand heavier tests, for instance that with the ram of 1,015 kilogrammes (2,240 lbs) the steel requires to be of a somewhat different

composition, and this leads me to consider the influence of manganese on phosphorus steel.

Heights of fall in feet.	Bessemer Steel. Deflections in inches.	Phosphorus Steel. Deflections in inches.
1.5	0.03	0.07
2.5	0.06	0.13
3.	0.14	0.31
5.	0.30	0.47
6.5	0.50	0.91
8.	0.90	1.32
10.	1.30	1.80
Break.	12 to 14 feet.	11 to 12 feet.

We have said that, in the manufacture of soft steel, 1 per cent. of manganese in the form of ferro-manganese of 50 per cent. was sufficient to obtain a suitable quality, and that if richer alloys, say of 60 to 70 per cent. of manganese be used, the proportion of carbon would be still less. When we have to deal with phosphorus qualities, and use 1½ per cent. of manganese with rich alloys of 60 or 75 per cent., there is an excess of manganese, and the metal may retain as much as 1 per cent. of it. The rolling does not seem to be affected; on the contrary, it seems to be rather easier; but what is most remarkable is that phosphorus steel shows a considerable increase in the power of resistance to shocks. The effect of the phosphorus seems to be neutralized by the manganese. I may particularly mention the experiments made at St. Petersburg, for converting light iron rails of English make into steel. These experiments were carried on in the works of the Grande Société de Chemins de fer Russes. What conclusion, from a practical point of view, may be drawn from these properties of soft steel containing from 0.3 to 0.5 of phosphorus? Pure ores are rather scarce, and are likely to become exhausted, and of late years their price has been nearly trebled, owing to the excessive demand. This means, that for common steel rails, for instance, a cheaper means of manufacture is wanted, in addition to an almost unlimited power of production. In the dre process of using a bath of pure pig iron, mixtures may be employed containing 0.3 to 0.4 per cent. of phosphorus. Most English pig iron, except that of Cleveland, is thus treated, and this may lead to a considerable reduction in the cost price. On the other hand, the considerable increase in the use of steel rails consumes a large quantity of old iron rails which have been almost without value. The question of the remanufacture of old materials into steel is sure, in the future, to be brought prominently under the notice of the railway companies. This operation can be performed in the Siemens-Martin process at a cost which compares favorably with that of the Bessemer process.

3d. MANUFACTURE OF MANGANESE STEEL.

Of late years, attempts have been made to apply the term "steel" to certain alloys in which carbon was replaced by sundry simple bodies, such as tungsten, chromium, silicon, boron, manganese, &c. Experience has not shown, however, that such a generalization is warranted. If fusibility, drawing out, and sensitive tempering, are taken as the distinctive qualities of steel, it may be stated that manganese steel exists also. An instance of this may be given in the following analysis:

Carbon	0.38
Manganese	1.38
Iron	98.24
	100.00

This steel is a fine-grained body, whitish gray and very brilliant. Under cold hammering it may be slightly drawn, but cracks at the angles; when red hot, it is as ductile as iron, and is very soft; when white hot it can easily be forged, and can be welded together without artificial means. If it is tempered in water when bright red, the surface scales and becomes as hard as quartz, then it becomes brittle, and the fracture is more brilliant, almost white, and without the bluish glitter. These are, if I mistake not, the properties of a real steel, but notwithstanding a small proportion of carbon, if it were not for the manganese, would place it in the class of soft metals, which cannot be obtained by spiegel. This is, therefore, a new body, which will in the future play a prominent part in metallurgy.

Let us suppose that in the Bessemer or the Siemens-Martin process, and with bodies of ordinary purity, 1½ per cent. of manganese is added by means of ferro-manganese of rich yield, 60 to 75 per cent. for instance. Experience has

shown that only $\frac{1}{2}$ per cent. is necessary for the reduction of the oxyde of iron, and that the metal retains about 1 per cent. of manganese, with not more than 0.2 per cent. of carbon. The following are the results of tensile tests made with four different smeltings of this metal with 1 per cent of manganese :

	Tons per square inch. A.	B.	C.	D.
	Tons.	Tons.	Tons.	Tons.
Limit of Elasticity	18.4	18.1	18.8	21.7
Load of breakage	35.1	34.6	34.0	37.1
Elongation per cent. over 8 inches	20.0	21.0	20.0	21.77
Elongation per cent. over 4 inches	25.5	25.3	25.0	28.80

This same metal, when at a cherry-red heat, and tempered in water, bears an average breaking weight of 48 to 50 tons per square inch, with an elongation of 4 per cent. measured over a length of eight inches. Its most important characteristic—as may be gleaned by analogy from what has been said above, relative to the phosphorus manganese metal—is its resistance power to shocks. Axles, for instance, have to withstand bending to the extent of five inches, and then to straighten themselves again, the test being made on the centre of the axle, the diameter of which, where the weight falls, is four inches. This, as manufacturers are well aware, is a very severe test, and it is only good metal that can withstand it successfully. It can, however, be said that when 1 per cent. of manganese is added to a steel produced in the manner already described, not only is the test withstood once, but the operation may be repeated several times, and the axle will spring to its original position. Although this metal is not yet well known, that is as far as regards all the uses for which it is applicable, it must be acknowledged that there exists in it a superior quality, which it is easy to give to all steel.

The judicious combination and treatment of the different elements which are at the disposal of man will attract more attention in the future, but only little is known on this question at present. Amongst the elements, which may be most readily used for admixture, is manganese—it is found most plentifully, as a mineral product, and is a most active element.

We must again refer to this influence of manganese because of its utility in the production of steel, and may say that the conclusions arrived at have been come to by reasoning of the most veritable certainty.

In certain cases where manganese is found in small quantities, and is partly eliminated in the successive operations, it may be stated that the perfection of chemical analysis did not admit of the detection of the small proportion of manganese contained in an excess of iron. Manganese is now found in the best steel, where its presence formely was not even suspected. It may now be affirmed that this "steeling" propensity of certain minerals is now available for all districts where they do not exist in a state of nature, under the simple and useful form of ferro-manganese.

THE DECAY OF BUILDING STONES.

By C. P. TOWNSLEY, C. E.

Written for Van Nostrand's Magazine.

In the selection of building materials there are two important requisites, durability and color. Both of these should be combined to make a perfect structure, but the color having only to do with external appearance, is not the point which principally concerns the engineer. He should ascertain the durability, and let color only decide between materials otherwise equal.

In building stone, there are two principal sources of decay, those inherent in the stone itself, and those due to external circumstances. Of those causes inherent in the stone itself, we have first : a want of cohesion among the particles

and porosity, which, in many cases, is a direct consequence of the former. This want of proper cohesion may be due to the absence of a cementing medium, or the stone may not have been exposed to sufficient pressure to consolidate the mass. The result is a weak stone, which, if porous, absorbs much water, and is disintegrated by frost—a most objectionable feature. Too much argillaceous matter is also unfortunate, whether it occurs in seams or distributed through the mass. When in seams we find lines of weakness owing to their better absorbing power. Thus distributed it causes brittleness, and is rapidly acted upon by the weather. Iron pyrites is another, and a most frequent cause of decay. If found in nodules, or in large crystals, its effect is not so disastrous, as when in fine particles distributed through the stone. Ferrodus compounds are also deleterious, as is seen in gray freestone, which is shown by the accompanying table not to be a durable stone.

The effects of iron pyrites seem to be more noticeable among the limestones than any other of the building materials. Prof. James Hall, in his report on "Building Stones to the Capital Commission," says: "If magnesia be present the sulphuric acid formed by the decomposing pyrites produces a soluble efflorescent salt, which exudes to the surface and forms white patches, which are alternately washed off and replaced, but leaving a whitened surface, probably from the presence of sulphate of lime. If the stone be calcareous, the salt formed is insoluble, and therefore produces less obvious results."

The lime from the mortar may produce a similar effect, when the adjacent stone contains pyrites.

The size of the constituent particles or crystals of a stone may affect its durability. This is not always the case, for we find varieties of crystalline marbles which are much superior in strength and durability to those of finer grain. A mixture of fine grains of sand, with pebbles of various sizes, can seldom be relied upon.

The integrity of a stone depends largely upon the character of the cementing material. A clayey medium is not good as it absorbs much water, and is injuriously acted upon by the frost. A calcareous cement is dissolved by rain water, charged with carbonic-dioxyde, while a silicious cement is undoubtedly the most durable, as weathering seems to have little or no effect upon silicates.

The action of frost is first to be considered. This alternate freezing and thawing is, in a climate so changeable as ours, the most trying of any or all other conditions to which a stone is subjected. It is then to this particular point we wish to direct special attention. There are certain general facts by which we may select the durable from the non-durable stones. We know that loose and friable stones, porous, and those with a superabundance of argillaceous matter are severely tried by the action of frost, but there is need of a definite test, and when we consider how very few of our public buildings, erected of stone, have stood for fifty years, we shall be better prepared to appreciate the dilapidation and ruin which must ensue during the next century, and see the great necessity of guarding against its principal cause, the action of frost.

An artificial test for the action of frost has been recommended by M. Brand, a French chemist, which appears to have met with better success than any other method. This consists in procuring a specimen of the stone to be tested of about a two inch cube, then preparing a cold saturated solution of sodic sulphate. Boil the specimen in this solution for thirty minutes, so as to saturate the stone as completely as possible with the salt, then remove the specimen from the fluid, and suspend it in a cool damp cellar over a dish containing some of the solution of the salt, carefully freed from all sediment by filtration. An efflorescence of the sodic sulphate will soon appear upon the stone, when it should be dipped into the solution below, and allowed to remain for some time (until all bubbles of air have disappeared from the surface). The stone is again suspended as before. The process is repeated for a week or more, and the amount of earthy material found in the vessel below will show the action of the sulphate, or the corresponding action of frost. This result, compared with those of similar experiments on other stones, will show the relative action of frost.

The following is a table of some of

the building stones of our own country, with the result I obtained by subjecting them to this artificial test. They were exposed to the alternate action of the solution and air, as above described, for fourteen days:*

	Sp. gr.	Per cent. loss in weight.	Comparative resistance to the action of frost.
Trenton Limestone, N. Y.	2.76	.000286	1.
Onondaga Limestone, N. Y.	2.73	.000363	.7878
Dayton Limestone, Ohio	2.71	.000757	.3778
Blue Limestone, Schenectady, N. Y.	2.55	.000831	.3441
Magnesian Limestone, New Jersey	2.84	.001517	.1884
Granular Limestone, Eastern N. Y.	2.82	.001579	.1811
Columbian Marble, Vt.	2.74	.000404	.7079
Black Italian Marble, Vt.	3.09	.000506	.5652
White Italian Marble, Vt.	2.78	.000522	.5478
Gray Limestone Marble, Salisbury, Ct.	2.97	.000525	.5437
White Marble, Salisbury, Ct.	2.86	.000529	.5406
Irish Marble	2.71	.000625	.4576
Rutland Marble, Vt.	2.71	.000696	.4109
Red Granite	2.65	.000487	.5872
Gray Granite, Barre, Mass.	2.63	.000682	.4193
Gray Granite, Keene, N. H.	2.61	.000873	.3276
Hallowell, Me.	2.64	.000921	.3105
Fox Island, Me.	2.63	.001198	.2389
Black Granite, Haddon, Ct.	2.78	.001203	.2360
Luzerne Gneiss, N. Y.	2.18	.000548	.5218
Gray Gneiss, N. Y.	2.61	.000858	.3333
Syenitic Gneiss, Warren Co., N. Y.	2.53	.001152	.2408
Gneiss from Moreau, Warren Co., N. Y.	2.68	.001523	.1878
Indiana Sandstone	2.48	.000813	.3517
New Jersey Sandstone	2.40	.000911	.3139
Potsdam Sandstone, Malone, N. Y.	2.49	.000953	.3001
Berea, Sandstone, Ohio	2.40	.001132	.2526
Portland Sandstone, Ct.	2.35	.001201	.2373
Basaltic Rock	2.76	.000619	.4620
Semi-metamorphic Rock, near Hoosac	2.96	.001057	.2706
Brick made at Schenectady	1.92	.002168	.1319
Calcareous Tufa, Mumford, N. Y.		.002356	.1214

The Trenton limestone appears to have given the best results (this specimen, however, was much above the average of this kind of stone), next came the Onondaga, while a calcareous tufa of which calcic carbonate forms the greatest amount, gives the poorest results. The specimens of Trenton and Onondaga limestones were of a very close and fine texture, while the tufa was very porous, indeed, nearly as much so as pumice stone. All of the marbles, and all but two of the limestones, gave very good results. The Magnesian limestone was of a very loose structure, and could be crumbled off in grains with the fingers. The granular limestone, though not so loose as the magnesian, was of very much the same character. As there was a flaw in the specimen, the result cannot be relied upon.

The Granites all showed good results. The red granite, which is by experience one of the very best for durability, appeared superior to all others. The Fox Island was not so good, it being coarse grained. The specimen of Indiana sandstone stood the test better than any of the sandstones. It should be remembered that this kind of stone is not uniform in texture, for while in some localities it furnishes a durable stone, in others it is

* These experiments were conducted at the Chemical Laboratory of Union College, Schenectady, N. Y.

quite inferior. The specimen used was above the average.

The limestones which, in the table appear superior to the granites, are not necessarily so, for while the experiment was simply to test the action of frost, there are other agencies which serve to destroy these stones, such as water charged with different solvents. The poor comparison which many of the stones show, should not be taken as showing a stone entirely unfit for building purposes. The calcareous tufa has stood six years in a church at Mumford without apparent signs of decay. It is not, however, a commendable stone.

The destructive action of the frost can be modified by the manner of laying the stone, which should be placed in the building as they were originally in the ground. This is particularly applicable to the sandstones, and our brown freestone is often laid without regard to this law, many of our buildings suffering severely from this neglect.

In and about our cities we have gases given off during the combustion of bituminous coal in particular, which are especially corrosive in their action upon building stones. The granites which have withstood the weathering of centuries in the pure air of the Egyptian deserts, when brought to Paris or London soon lose their sharpness of outline. Limestones and calcareous sandstones suffer more than others, and in the choice of building material, special regard should be paid to the exposure.

The resistance of stone to crushing is a subject of growing interest, and has been thoroughly investigated by Gen. Q. A. Gilmore. The average of ninety specimens of granite, which he has tested, is about 17,500 pounds to the square inch; of forty-three limestones, the average is about 11,000 pounds; of twelve marbles, about 12,000 pounds; of sixty-two specimens of sandstones, it is about 8,500 pounds to the square inch. This gives to the granites a very great difference; still, in some of the most remarkble structures in Europe, limestones are used to support great weights. The pillars supporting the dome of St. Peter's Church at Rome, that of St. Paul's in London, and St. Genevieve in Paris, are all of limestone.

After studying the causes of decay, a means of remedying them must next be considered. There have been many methods recommended for the preservation of stone, and the almost invariable idea has been to render the stone non-absorbent. This has been attempted by combining certain portions of mineral substances, with oil or fatty matters. These, however, all tend to decompose, and when to avoid this difficulty pitchy or bituminous matter is used, the compound is so dark and unsightly as to preclude its use. It is generally conceded that any mechanical mixture not involving a chemical combination must fail. Ransom's process seems to have met with as good success as any other.

THE COMPLETE BESSEMER PROCESS.

From "Iron."

THE session of the Iron and Steel Institute which has just concluded, has indeed hardly been marked by any paper of remarkable interest or novelty. Such exciting questions as mechanical puddling and the details of blast-furnace economy, which have agitated previous meetings so profoundly, have only been lightly touched on. In fact, the meeting would have been inferior to many of its predecessors in technical importance had not its character been redeemed by the animated discussion which, by a felicitous innovation, was introduced by the manager of the Barror Iron and Steel Works giving his experience of the "use of molten iron direct from the blast-furnace for Bessemer purposes." It is hardly too much to say that the result of the conversation thus started is to establish a new point of departure for one of the most valuable processes known to metallurgy.

The history of the origin and develop-

ment of Bessemer's famous invention is, perhaps, one of the most instructive in that curious and generally gloomy record, which tells how great ideas have been, worked out for the benefit of the world by the devotion of men who have rarely reaped the fruit of their labors. The founder of the largest modern steel industry had a peculiar experience, in that, unlike most inventors, he has not only earned but obtained his reward; and now, just twenty years after the reading of his paper on "The production of malleable iron and steel without fuel," before the incredulous *savants* of the British Association, he is taking part in a series of addresses—for it was hardly a discussion—which establish the superiority of a mode of working which was claimed at Cheltenham, but afterwards abandoned in England till re-introduced from the Continent.

It has been well said that valuable inventions often fall to the ground, simply because the technical knowledge of the day is not up to their level. They are born prematurely into a world which has not the capacity necessary to profit by them, and remain dormant till rediscovered by some one who has not the ill fortune to be in advance of his age. We should otherwise be inclined to say, that the interposition of the cupola furnace between the blast-furnace and the converter, was simply one of that chapter of accidents which attended the first years of the Baxter-House heresy. It appears that when Bessemer began to experiment on the decarburation of cast iron by the injection of air, he did not have access to a blast-furnace, so he was compelled to have recourse to a melting furnace to bring the pig into a molten condition. Whether he would have persevered in his labors if it had not fortunately happened that the first metal be experimented on was good Blaenavon pig, and therefore amenable to atmospheric treatment, seems open to question. But the experiments were successful, and the cupola was associated with the success. Then came the trying period. At Dowlais Mr. Menelaus placed a blast furnace at the inventor's disposal, and direct tapping—or something very like it—was tried. But the pig was bad, and the product worthless. The president of the Institute created much amusement by explaining, with mock indignation, that the inculpated pig was not of normal Dowlais quality, but only tolerated as good enough "for American railway iron." At all events, after this in the subsequent developments of the pneumatic process, the cupola was fallen back on, without any vigorous attempt to make the direct process a success.

It must, however, be recollected that twenty years ago empiricism reigned supreme in the blast-furnace practice of nine ironworks out of ten, and even those which were cautiously calling in the aid of chemical science, had neither the command of ores nor the technical experience, which nowadays render it comparatively easy to produce with regularity any particular quality of pig for which there is a sufficient demand. A uniform quality of pig is essential for the successful working of the Bessemer process; and the roundabout pig-and-cupola system offered a rough, though expensive, solution of the difficulty, by affording facilities for mixing different brands of pig so as to obtain a pretty constant mean product.

Yet, if we consider the matter in the light of our present knowledge, it certainly seems strange—not to say discreditable—that English metallurgists should have been nearly a generation in adopting a mode of working which was in successful use at Neuberg in Styria in 1865, and in France and Sweden several years ago, and of which, moreover, the *prima facie* advantages are patent. The Bessemer process starts with melted pig-iron; and melted pig-iron is precisely the substance which is tapped from the blast-furnace. The obvious course would seem to be, either to place the converter close to the blast-furnace and let the molten metal run in direct; or, if this were inconvenient, to tap a vessel which can be readily discharged into the converter. Common sense revolts against the idea of first running the pig into sand-moulds to cool, and take up an injurious incrustation of sand, and then laboriously removing the cooled pig to be melted again at the cost of expensive fuel. Yet this is what we have, without exception, been doing till quite lately; and, so recently as the Barrow Meeting of the Institute, there were found able and experienced *steelmasters*, who vehe-

mently ridiculed the idea of its being possible to do otherwise. We will not recall all the arguments that were then used against an innovation which has now the intelligent and candid support of those who then depreciated it, but content ourselves with considering the one point in which it is still contended that the use of remelted pig is advantageous. The heat which maintains the contents of the converter in a state of perfect fluidity, is due to the oxydation of carbon and silicon; some of the iron and any manganese present also contributing by their oxydation to the total calorific effect. The calorific power of silicon being very high, it depends on the amount of silicon present in the pig, whether the converter charge blows hot forming a perfectly liquid mass—or not. If there be too little silicon present, the blow will be prolonged, and there will be a constant formation of *skulls*! or solidified metal attaching to the sides of the vessel. If, on the other hand, there be too much silicon present, there will be an excessive loss of iron, as slag; and there is a difficulty in completely burning out the silicon by the time the carbon is removed. It would appear that from 1½ to 2½ per cent. of silicon is an essential constituent of good Bessemer pig, but that a much larger proportion than this is injurious. Here we have the sole valid argument for the retention of the intermediate melting by which the silicon is in part eliminated. But this is only when dealing with very silicious pig, and, by a careful regulation of charges in the blast-furnace, there is no reason why the percentage of silicon should not be kept within such bounds as to render the use of this subsidiary desilicizing process quite unnecessary. In fact, the reverse necessity sometimes arises. In the South of France we find ironmasters driven to discard the remelting system, by the fact that their slightly silicious metal lost so much silicon in the cupola as to render it unfit for the converter. Mr. Snelus, of the West Cumberland Ironworks, finds that, while with the pig-and-cupola process it takes only 23½ cwt. of metal to produce a ton of steel ingots, it requires of the direct-tapped metal about 24 cwt. This half-hundred weight deficiency is ascribed to the presence of too much silicon; but we can hardly accept this as another than an exceptionally unfavorable case.

As all metallurgical improvements ultimately resolve themselves into questions of comparative cost, it is well to inquire what is likely to be the pecuniary value of the proposed modification of our Bessemer practice. The only estimate given on this point was that of Mr. Snelus, who reckoned the saving at from four to five shillings a ton. Considering that this is at works which were not originally planned for direct working, it may be taken as a safe minimum. Mr. Smith, of Barrow, was less confident as to the saving effected at these monster works, but as, owing to local peculiarities, the molten metal has here to be carried nearly two miles to the converters, it is rather to be wondered at that any success has been attained at all. Indeed, the facility with which it is found that fluid cast iron can be handled and retained in a liquid condition for long periods of time opens quite a new field for technical application. But what is of greatest importance, is the general testimony borne to the superior quality of direct-tapped steel. As quantity is the ideal of blast-furnace managers, quality must always be the first consideration of steelmakers. The immediate effect of these discussions will probably be to render the alliance between iron-smelting and steel-making even closer than it has hitherto been. Those steel-makers who depend on supplies of purchased pig will be placed at a disadvantage, as compared with their rivals who have blast-furnaces on the same ground as their converters, certainly to the extent of some shillings a ton. The result will probably be that all Bessemer pig-makers will gradually become steelmakers as well. At first sight it would appear that, if direct tapping is economical in the Bessemer proeess, it must be equally so in the Siemens-Martin steel furnace. But here we are met by the unexpected fact—vouched for by Mr. Hackney—that the decarburization of a charge of previously melted pig-iron by this system takes quite as long as if the pig were charged in an unmelted state.

The world is indebted to England for Bessemer and Mushet, who invented and rendered available a mode of producing

steel—the most valuable of the metals—on such a scale as to enable it to take the place it deserves among the potent instruments of civilization. But it is only fair we should acknowledge that it is in Sweden and Austria, in France and Belgium, that we have learned that the preliminaries to the actual process may be greatly simplified. We have yet fully to recognize that America has much to teach us in the most advantageous conduct of the process and the construction of a plant, for which they are ready enough to admit they are indebted to British enterprise and ingenuity. But, while our ironmasters continue to show themselves as ready both to learn and to teach as we have seen them during the past week, we need not fear for our iron manufactures.

MANGANESE BRONZE.

From "Engineering."

Mr. P. M. Parsons, well known in connection with the conversion of cast-iron guns into rifled ordnance, as well as for the peculiar metal, which under the name of "white brass" is very largely employed for bearings and other purposes, has recently produced an alloy, which promises to play an important part as a constructive material.

This alloy, called manganese bronze, is formed by incorporating manganese with the various bronze mixtures, with the object of removing any oxyde existing in the metal, by means of the strong affinity of manganese for oxygen. The action of the manganese in the alloy is strikingly visible in the texture of the metal, a fracture of which so far from presenting the coarse granular appearance characteristic of ordinary bronze, is as closely and finely grained as the best qualities of steel, while the strength and tenacity of the alloy is greatly increased, as has been shown by a series of tests lately carried out at the Royal Gun Factory, Woolwich.

Another singular and valuable quality which this metal possesses is the facility with which it may be forged at red heat, these operations greatly increasing its strength and toughness.

The qualities of the metal were tested by six specimens, three of which were cast and the other three forged, the series representing three different degrees of hardness. These samples were proved to ascertain the tensile strength, elastic limits, and ultimate elongation, and the following results were obtained.

1. A cast specimen of tough quality adapted especially for constructive purposes, showed an ultimate strength of 24.3 tons per square inch, with an elastic limit of 14 tons, and an elongation of 8.75 per cent.

2. The same quality forged had an ultimate resistance of 29 tons per square inch, an elastic limit of 12 tons, and an elongation of 31.8 per cent.

3. This was a cast sample of harder quality. It broke under a load of 22.1 tons per square inch, had an elastic limit of 14 tons, and an elongation of 5.5 per cent.

4. The ultimate strength of the same quality when forged, rose to 28.8 tons per inch, the elastic limit was reached at 13.2 tons, and the elongation was 35.3 per cent.

5. This sample was still harder. The cast test piece broke at 23.6 tons, with an elastic limit of 16.8 tons, and an elongation of 3.8 per cent.

6. The same quality forged, had an ultimate strength of 30.3 tons per inch, the elastic limit being reached at 12 tons, and the elongation being 20.75 per cent.

From these truly remarkable results it will be seen that the first sample showed an ultimate strength equal to good wrought iron, while it is greatly superior to the best gun-metal, for which 16 tons per square inch with an elastic limit of 7 tons are extremely high strengths. The effect of forging this metal is also very striking, raising, as it does, the strength to such high limits, and practically giving

a new and most valuable metal to the world. In some respects, indeed, it resembles aluminum bronze, but its resistance greatly exceeds that of this alloy, which does not exceed 22.6 tons per square inch, while its elongation and elasticity are considerably inferior to that of the manganese bronze.

It would be superfluous to indicate the wide field of usefulness in which this metal may be applied. Manganese bronze, by virtue of greater strength and more reliable character, will find an application wherever gun-metal is employed, while the facility with which it can be forged, and the benefit it derives from this operation, will render it still more useful as a constructive material.

HYDRAULIC EXPERIMENTS AT ROORKEE.

By Capt. ALLAN CUNNINGHAM, R. E.

From Professional Papers on Indian Engineering.

The experiments referred to, in the following brief abstract of Capt. Cunningham's report, were conducted during the winter of 1874–5, for the purpose of contributing towards a knowledge of the laws governing the relation between the discharge of rivers and the measured velocities.

As the facilities for such measurement were unusually good, while the skill and care of the experimenter were equally so, the record of the work is certainly a desirable addition to current engineering literature.

We quote directly from the report:

The experiments were performed in the Ganges Canal, which, in the neighborhood of Roorkee, presents unusually favorable opportunities for such experiments in the occurrence of *a straight reach of six miles* of comparatively uniform section, from Dhanouri to Roorkee, the only obstructions in the current above the Solání Aqueduct being two narrow piers of two bridges at Pírán Kalliar and Mahewa.

This six-mile reach comprises three descriptions of channel, viz.:

i. *Trapezoidal Channel*, in earth, 3 miles long, 150′ average bed width.
ii. *Trapezoidal Channel*, masonry sides (steps), clay bed, 2 miles long, 150′ bed width.
iii. *Rectangular Twin Channels*, in masonry, 932′ long, each 85′ wide.
iv. *Trapezoidal Channel*, same as ii, ½-mile long.

Thus the *same body of water* passes successively through these different channels, each of which is nearly uniform throughout its own length, but differing from each other in material, figure of cross-section, and width—thus presenting a very favorable locality for Hydraulic Experiments.

In order to establish confidence in the Results of these Experiments, it seems advisable to explain in detail the Instruments and mode of observations used for measurement of velocity.

Many instruments have been at various times proposed for this purpose: they may be roughly classed as Fixed Instruments and Free Instruments.

I. Fixed Instruments.—Such are the Tachometer or Current-metre, Pitot's Tube, Hydrometric Pendulum, Water-lever, Water-vane, Hydraulic Balance, Rheometer, &c., &c., which *being fixed in a given position* measure directly or indirectly the current-velocity. Several of these are described in Weisbach's "Mechanics of Engineering," Vol. I., Art. 378, *et seq.*, and in the "Mississippi Report," page 202, *et seq.*

Most of these Instruments—except perhaps "Pitot's Tube" are open to numerous objections. "Pitot's Tube"—as improved by Darcy—has become almost classical from its nearly exclusive use in Darcy and Bazin's Experiments.

The delicacy of observation, however requisite, with all these Instruments—except the Current-metre—necessitates their being used from a very *steady temporary bridge*, the erection of which over a wide channel would of course be impracticable.

Elliott's and other "Current-metres" can be used *from a boat*, but the presence of the boat so greatly modifies the natural motion of the water, that good results could not be expected from them *within a distance of some feet below the boat's keel.*

[Some of the "Fixed Instruments" (*e.g.*, the Current-metre)—in consequence of being provided with a large "tail" which causes them *always to face the current*—measure (or are intended to measure) the actual velocity of the fluid filaments passing them, and not merely "velocity" as defined in Art. 8, viz., the resolved part of the actual velocity parallel to the axis of the stream; but this is on the whole rather a disadvantage (see Art. 8) than an advantage in their use.]

For these reasons, the use of "Fixed Instruments was entirely rejected in these Experiments.

[It may be remarked that for similar reasons they were entirely rejected in the Mississippi Experiments.]

II. Free Instruments, or Floats.—By this term is meant any sort of "floating apparatus," which may for shortness be called a Float, whether floating at the surface, or partly submerged—which is dropped into, and abandoned to the current. In a "uniform stream" all such objects acquire after a time a state of *relative equilibrium* in which their velocity is tolerably uniform. After this "terminal velocity" has been acquired, the Time of passage of the Instrument across the space between two parallel cross-sections at a known distance apart, is carefully timed by one or more chronometers.

It must be observed that the actual Result obtained by this sort of observation is not the "velocity" at a particular point, either of the fluid or even of the "Float" itself—using the term "velocity" in the sense of Art. 8—but is really only the

"Average or mean of the 'velocities' of the 'Float' itself taken over the measured distance, which is expressed mathematically by $\left(\int_0^x v\,dx\right) \div x$," (2),

and it is assumed as nearly certain, that this is *the same as the average of the "velocities" of a fluid particle* over the same distance.

[For this reason, these "*Floats*" can only be used with any advantage in (so called) "uniform streams." In variable streams, a fixed instrument would be preferable when practicable].

The knowledge of this quantity, although not nearly so valuable as that of the "velocity"—both in direction and magnitude—of the fluid at a particular point would be for the important object of unraveling the laws of motion of fluid, is however almost as valuable for all practical purposes in calculating simply the "Discharge."

These Instruments can be used from a boat, and are therefore the most suitable for use in a very wide channel—except *very close to the edges*, where a "Fixed Instrument" would be preferable (see Art. 35). They were almost exclusively used in the Mississippi Experiments, and have been also *exclusively used in these Experiments.*

With these Instruments the following conditions should be fulfilled:

1°. The whole Instrument in all its dimensions should be so small, as to disturb the natural motion of the water as little as possible.

2°. The dimensions of the parts of the Instrument should nowhere exceed a breadth and depth so small, that the "velocity" of the current is sensibly uniform throughout that breadth and depth.

[In the present experiments, these dimensions were fixed at a maximum of $3'' \times 3''$ for general use, but this *is too large near the edges.*]

3°. The parts of the Instrument should be so arranged, as to severally expose a constant surface (both directly and laterally) to the current, *however the Instrument turns* during the motion—(after the "terminal velocity" has been acquired).

4°. The Instrument should expose as little surface as possible to the wind.

5°. It should be strong enough and simple enough to bear moderately rough handling, and should be convenient to handle.

6°. It should be cheap enough to admit of being made in large numbers.

[In consequence of want of experience as to the best form of Instrument for velocity-measurement in a large Canal, the author decided from the first to use only the simplest and most inexpensive, of such material as could be easily procured, and of such make up as could be readily executed in a small native bazar. This in part led to the sole use of "Free Instruments" (Floats) made of wood.

The experience now gained has pointed out the directions in which more expensive materials and better workmanship may be advantageously employed.]

13. OBJECTIONS TO FLOATS.—As a spirited attack has been made by Mr. Révy (see Révy report *passim*) on the use of Floats, utterly condemning their use at any rate on Great Rivers, and especially for subsurface-velocity measurement, it seems essential to notice it here. Mr. Révy goes so far as to say of the Mississippi Survey—(see page 8 of Révy Report.)

> "The Engineeers of that Survey relied entirely on floats, and we consider it a misfortune to science and to practical Engineering that so much ability, perseverance, and time should have been spent to obtain results which the unfortunate choice of floats has inconveniently marred and confused."

This is a pretty decided condemnation of the use of Floats.

Mr. Révy admits, however, (p. 6,) that—"under favorable circumstances the velocity of the surface current may be observed by the movement of a float with considerable accuracy." Suffice it to say, as far as regards Experiments on a straight uniform Canal, the circumstances (which he points out as necessary to use of floats, are highly favorable.)

[Mr. Révy, however, considers a down-stream wind of equal velocity with the surface-current essential to accurate use of Floats. This is quite necessary in the Author's opinion: provided the floats do not project sensibly above the surface (compared with the mass buried below the surface)—a condition easily fulfilled on Canals and small rivers,—they will move sensibly along with the local surface-current, (which is itself of course affected by the wind). A calm or a simple up or down-stream wind would not affect the accuracy of the observations. A cross-wind would undoubtedly interfere seriously with the use of floats; but so it would with useful work with any form of Instrument].

Mr. Révy's principal objections are however to the use of Subsurface Floats.

Nevertheless the use of these Floats enabled the Mississippi Experimenters to clearly establish (page 225—262 of Mississippi Report).

1°. That the line of maximum velocity is generally below the surface.

2°. That its position depends on the wind.

[A law of dependence was even proposed.]

Now both these Results are fully verified by the present Experiments conducted solely with Floats, and Result 1° is also fully borne out by the Darcy-Bazin Experiments, performed solely with a different instrument (a Pitot's Tube).

It can be verified at once by any one for himself by simply throwing a chip of wood and a common beer-bottle filled with water enough to sink it pretty deeply into a current together, when the bottle will be found as a rule to move the quicker. It has also been long known to watermen that a deeply laden barge floats more quickly down-stream than a lightly laden one.

Result No. 1° (above) may be said to be one of the best established Results of modern Hydraulic Science.

The Instrument used by Mr. Révy—a Current-metre—in preference to the Floats, which he condemns, *did not enable him to recognize this so easily established fact at all.* He declares (p. 87), relying on his own Experiments with the Current-Metre, that the maximum velocity line is at the Surface, and argues (p. 87), that it *ought to be so.* The inference would seem to be that this Instrument (Current-metre) is not so delicate an Instrument for velocity-measurement as the (condemned) Floats.

Suffice it to say here, that the objections (though serious enough) are—in the author's opinion after this season's experience—by no means so insuperable (at any rate as applied to Canals not exceeding 9 feet in depth) as Mr. Révy considers. These objections would be undoubtedly aggravated at greater depths.

But is the Current-metre proposed by

Mr. Révy *more trustworthy* than the Sub-surface Floats? Comparative Experiments alone could decide this. Mr. Révy gives none.

Omitting the detailed account of the experiments, we give herewith the writer's summary of results:

For facility of reference a summary of the results of this paper are here collected: the results are not arranged in the order in which they occur in the text, but in that which seemed most convenient for exhibiting the results at one view:

The term Velocity in Hydraulics means usually only the Resolved part of the Actual Velocity parallel to the axis of the current; for mere discharge-measurement this is a more useful quantity than the actual Velocity.

Fixed Instruments are—in consequence of the delicacy of observation required—in general (the "Current Metre" excepted) suitable only to positions where a steady support can be secured; *i. e.*, only to small streams, and to observations close to margin in large streams.

Free Instruments are alone suited in general to large streams.

Free Instruments do not measure the Velocity at a point but only the Average Velocity (the term Velocity being taken as above defined) along the stream-line throughout their run: for mere discharge-measurement this is quite as useful a quantity as the resolved part of the actual Velocity at a point would be.

No "Free Instrument" yet invented for measurement of subsurface-velocity in a large stream is quite satisfactory.

The conditions which should be fulfilled by an Instrument of "direct observation" are essentially inconsistent, and can therefore be only partially satisfied together.

Instruments of "indirect observation" labor under the disadvantage of depending on some hypothesis of law of fluid pressure. Most "Fixed Instruments" have this disadvantage.

Of the four subsurface-instruments tried, and as constructed for these experiments:

The "Single Ball" fulfills all conditions except—(the most important for an instrument of "direct observation")—that its surface-float is not small enough for the effect of surface-action on it to be fairly negligible.

The "Tin" fulfills that condition in the highest possible degree, but fails in not exposing a constant surface directly and laterally to the current, and in not retaining itself at a constant known depth.

The "Twin Balls" depend for their utility on a hardly certain theory, and the observational requirements of that theory are very difficult to fulfill: they are, therefore, liable to more irregularity than the "Single Ball."

Wood is to hygroscopic to be a really suitable material for these instruments for delicate observations: metal would be preferable.

The "Single Ball" made in metal would probable be as good a subsurface "Free Instrument" as could be desired.

The "Rod" is not well suited for experiments to investigate laws of fluid motion,

and is useless as an Instrument of precision for measurement of mean velocity of a vertical plane;

but the discrepancy between the velocity of a rod and the mean velocity through its length is not so great as to interfere seriously with its practical use for discharge measurement.

The motion of a large body of water is unsteady, (even in a uniform channel of great length), *i. e.*, the velocity at a point varies considerably from instant to instant.

Single Velocity measurements are therefore incomparable unless simultaneous, a condition generally impracticable.

The Average Velocity at a point taken through a long interval of time, is sensibly constant under similar external conditions (of wind, depth, &c.)

Average-Velocity measurements derived from very numerous observations are therefore alone comparable, and not single measurements.

Hydraulic experiments in general must therefore necessarily be very tedious and consequently very expensive.

Comparative experiments with different instruments are therefore also very tedious.

Horizontal Velocity-Curves and Vertical Velocity-Curves depending on single

Velocity measurements are very irregular.

But the areas of the Surface Velocity-Curve and Mid-channel Vertical Velocity-Curve (computed from many ordinates *i. e.*, velocities measured—not simultaneously—but in succession) are approximately constant.

There is a probability, therefore, that the (superficial) discharges across the surface, and in the mid-channel Vertical plane are approximately constant.

The Surface-Velocity Mean Curve is strikingly regular and is symmetrical about mid-channel in a symmetric uniform channel of great length.

Its form depends on the figure of the cross-section.

In a very wide channel (breadth not $< 9 \times$ depth), it is a very flat curve.

In a rectangular section in masonry with a depth $= \frac{8}{9}$ of breadth it is approximately a "quartic ellipse" whose equation is

$$\frac{u^4}{u_0^4} + \frac{y^4}{b^4} = 1$$

The Surface-discharge in same case

$= .927 \times$ Central Surface-Velocity $\times$ Surface breadth.

The Surface-Velocity near the margin decreases very rapidly with proximity to the margin, and at the margin itself is extremely small, perhaps zero in case of straight margins of great length.

There is a constant surface motion from the margin towards the centre, most intense at the margin.

The Average Central Surface-Velocity varies at same place on a calm day nearly as the square root of the central depth, or as the square root of the hydraulic mean depth.

The Mid-channel Vertical-Velocity Mean Curve is strikingly regular, and is approximately a common parabola, whose equation is

$$(z - Z)^2 = p\,(V - v)$$

whose Axis is usually below the surface at a depth (Z) depending on the state of the wind,

and whose parameter increases very rapidly with the depth.

The Mid-channel Mid-depth Velocity is variable from instant to instant,

but the average of the same through a long time exceeds the Mid-channel Vertical Plane Mean Velocity (U) by a very small fraction; and is, therefore, convenient as a practical measure of that Mean Velocity.

The form of the Curve is hardly well enough determined (to be a *common* parabola) to admit of the Bottom Velocity being thence inferred.

The line of maximum Velocity of a Vertical Plane is highest at Mid-channel, and is more and more deep-seated further from Mid-channel.

The Velocity of a "Rod" is variable from instant to instant but its average velocity over a long time is sensibly constant.

Mean Velocity curves depending on single mean Velocity-measurements are irregular.

But their area is nearly constant, showing that the (cubic) discharge is nearly constant.

The Mean Velocity Mean Curve is very regular, and very much flatter than the Surface-Velocity Mean Curve.

The Mean Velocity of a rectangular section, $85' \times 9'$ in masonry, computed from Mean Velocity-measurements with (Rods) does not differ from the Mean Velocity of the Darcy-Bazin Results more than the probable observation-errors.

The present experiments lasting only four months, are of course only a small contribution to Experimental Hydraulics. It is to be hoped that they will be only the beginning of an extensive series of experiments in India, prolonged over several years under varied conditions on a large scale.

The Text will have shown numerous points on which further experiment is very desirable. But some limitation seems urgently required to prevent experiments being too diffuse, so as to secure some practically useful result within a reasonable time.

It might seem for instance desirable for the advancement of Hydraulic Science to aim at the discovery of the

> "Figure and Size of the 'Velocity-Surface' for the most 'useful cross-sections in masonry and earth in a "uniform Channel of great length,"

giving thereby the "velocity" (u) at any point of a cross-section as a function of

(*a*), the central surface velocity (u_o),
(*b*), the co-ordinates (y, z) of the point,
(*c*), the several dimensions (b, H, &c.,) of the cross-section.

So that in fact, f being now a known function

$$u = f(u_o, y, z, b, \text{H}, \&c.)$$

from which the Discharge would be at once calculable by reduction of the expression,

$$\text{D} = \iint u \, dy \, dz,$$

which might be done in a form suited to practical men once for all.

Nevertheless, it appears to the Author—after the experience of this season—that the above result could not be looked for within any reasonable Expenditure of time and money, unless some very much more expeditious mode of ascertaining the Average Velocity along a "stream line" can be discovered then by the use of Floats, as in these experiments.

[Thus it appears from Art. 33, that about a fortnight's work is necessary for the discovery of the Figure and Size of a single "Horizontal Velocity Mean Curve," so that the establishment of the Figure and Size of the whole "Velocity-Surface" would necessarily take many weeks, during which the canal must be maintained *of uniform depth of flow* at the chosen section].

This will be understood to be the consequence of the motion of water not being technically Steady, but only an Average Steady Motion.

[The important consequences of this were of course only gradually realized in the course of these experiments.

The following seems to the Author the best course for future experiments in order, to realize some practically useful results with a reasonable expenditure of time and money, viz.:

1°. "To verify whether the Mean Velocity past any vertical whatever is approximately the same as the mid-depth 'velocity' on that vertical."

2°. "To verify whether a 'Rod' sunk to nearly full depth measures approximately the Mean Velocity of its plane of motion, in any vertical plane whatever."

At present these two facts can only be said to have been verified (by the Mississippi Experiments, and by the present Experiments) *for the mid-channel plane.*

If either 1° or 2° can be verified *for all vertical planes* (as well as the Mid-channel plane), practically useful Results could be comparatively rapidly developed. Thus experiments would then be directed to ascertaining the

"Figure and Size of the Mean Velocity Mean Curve for the most useful Cross-sections in masonry and earth in a uniform channel of great length,"

giving thereby the Mean Velocity (U) *on any vertical line* as a function of

(*a*), the mid-channel Mean Velocity (U_o).
(*b*), the abscissæ (y) of the line.
(*c*), the several dimensions (b, H, &c.) of the cross-section.

Besides which, Experiments would be required towards determining the

Mid-channel Mean Velocity (U_o) as a function of the central Surface-Velocity (u_o) and of the several dimensions (b, H, &c.) of the cross-section.

So that in fact f, Φ being then known functions,

$$\text{U} = f(\text{U}_o, y, b, \text{H}, \&c.\ldots\ldots),$$
$$\text{U}_o = \Phi(u_o, \text{H}, \&c.)$$

From which U would be calculable in terms of the known dimensions (b, H, &c.) of the cross-section, and of a *single* (the central) surface-velocity (u_o) to be obtained *by actual observation.*

Such a Mean Curve could (it is believed) be generally determined *for any one central depth* with sufficient accuracy in about a fortnight's work, provided the canal could be kept running steadily at one level throughout that time.

For *each foot of central depth*, the Mean-Velocity Mean Curve should be separately determined.

This would complete the really necessary Experiments at one section.

An American manufactory has made 180,000 rifles for the Prussian Government, and is making 145,000 more.

THE BEHAVIOR OF METALS UNDER REPEATED STRAINS.

By LUDWIG SPANGENBERG, Professor der Königl. Gewerbe Akademie Zu Berlin.

Translated for VAN NOSTRAND'S ENGINEERING MAGAZINE.

II.

The fracture of iron extended only to the neutral axis, so that the compression-side had to be sawed apart. Above the neutral plane, on each vertical section, appeared a shell-like depression. From the fact that the compression-side bore the strain for a longer time than the tension-side, we infer that the absolute strength of wrought iron is less than its resilient resistance; contrary to the statements in most of the books. It does not occur to the writers that it is possible that, under repeated bending towards one side, the resilience of the compression side as well as its tenacity may gradually increase. The rupture of steel takes place through the entire section, apparently by tension, while the neutral plane is generally higher. This is due to the brittleness of the metal. But sometimes there appears on the upper edge a plane inclined 45° to the other rupture planes, which obviously has been caused by compression. A similar result appears in the axle steel, tested by falling weights; the section being perpendicular to the convex side, and forking at three-fourths the height, so that the piece forced out is in the shape of an equilateral triangle.

Phosphorbronze resembles steel in fracture; common bronze is like iron.

In the case of two steel rods under torsion, the rupture plane was divided into two entirely different parts by a chord perpendicular to the radius drawn to that point of fracture which was the centre of radiation. This is always on the tension-side in bent rods, so that it may be inferred that the rupture of axles under torsion is also due to tension, and not to compression. In iron rods under torsion, there are sometimes found two or more such chords and several centers. The rupture-surface is sometimes fibrous, sometimes globular. The fact that the smooth surfaces near the center of fracture are in general larger in proportion to the number of strains before rupture, *i. e.* in inverse ratio to the maximum tension; and the other fact that the center of fracture always lies on the tension-side, lead to this conclusion: that in consequence of repeated tensions the crystalline surfaces (joints) gradually become amorphous, so that several conditions of molecular equilibrium ensue; each corresponding to a limit of elasticity; that afterwards by continuance of the tensions the final limit is passed, and the resistance of the section is diminished so much that the force of the dynanometer is sufficient for rupture.

This hypothesis accounts for the appearance of the surfaces of rupture; *e. g.* for this; that the parts furthest from the centre of rupture have the same shining and crystalline look as rods broken by a suddenly applied strain. And this hypothesis is intimately connected with the "molecular constitution" of bodies, a theory now held by most physicists, and which is opposed to the old doctrine of the "homogeneity of masses."

The following principles appear in several works, especially in Moll's *Reine und Angew. Elem. Mechanik*, (Braunschweig, 1854); and in Redtenbacher's *Dymanidensystem* (Mannheim, 1857).

I. Molecular forces, attractive and repulsive, act in all bodies. The vehicles of these forces are atoms. Atoms are corporeal or ethereal, and exist simultaneously in every body.

(*a.*) The corporeal atoms of the body are inert and heavy, and mutually attract one another.

(*b.*) Corporeal and ethereal atoms mutually attract one another.

(*c.*) Ether atoms are inert, but not heavy (massive), and are so small in comparison with the corporeal atoms, and with the ethereal interspaces, that their form need not be regarded. The action between them is repulsive.

II. Among atoms the following forces operate:

(1.) *Universal gravitation; i. e.* the intensity of attraction between two corporeal atoms varies as the product of

their masses directly, and inversely as the square of their distance from each other; and is independent of their material constitution.

(2.) *Physical attraction.* By this is meant that force by virtue of which the same pair of corporeal atoms attract each other with a force varying directly as the product of their masses, and which diminishes very rapidly as the atoms are separated.

(3.) Chemical attraction, or affinity, by virtue of which two heterogeneous corporeal atoms attract each other.

(4.) *Ethereal forces.* Between ether atoms repulsion takes place; but between ether atoms and corporeal atoms there is attraction, varying directly as the product of the masses, and in a rapidly diminishing ratio of the distances.

III. Because of repulsion the weightless atoms of ether expand throughout space, penetrating all bodies, but concentrate more or less about corporeal atoms because of their attraction. Assuming that the distance between corporeal atoms is very great compared with their magnitude; and that the intensity of attraction between corporeal and ethereal atoms is very great compared with the repulsion between ether atoms; and that the number of ether atoms in a given volume is indefinitely greater than the number of corporeal: it is obvious that the ether will be disposed atmospherically about the corporeal atoms, and that each atmosphere will be of definite form and limit, so that a large part of the space between two corporeal atoms will be utterly void. It would also follow that the density of the ethereal envelop would decrease outward from the atom. Such an atom with its envelop is called a *Dynamid.*

IV. A molecule is a balanced group of two or more dissimilar corporeal atoms having a common ether envelop. As two distinct atoms can form a molecule A, so two like or unlike molecules may unite to form a compound molecule B.

V. Redtenbacher conjectures that the radial oscillations of ether atoms, which cause expansion of the envelop and increase of repulsion are connected with the phenomena of heat, while their continuous rotatory motion corresponds to the electric current.

The proposition of III, regarding the void spaces between atoms, we cannot reconcile with the hypothesis that ether fills entire space. We rather adopt Cauchy's view; that the intervening space is entirely filled with ether. This, Redtenbacher thinks, is the case only with solid substances, in which the corporeal atoms attract the ether atoms but feebly.

We shall now attempt to establish our hypothesis heretofore stated, by the application of these principles, and by the results of our experiments.

It is known that most, if not all, of the important technic metals show a tendency to crystallize when cooled from a melted to a solid condition (especially if the cooling be rapid). The atoms group about axes of symmetry, if unhindered. Otherwise a crystalline joint is formed. For example, if melted metal is poured into a cylindric vessel, made of a material which is a good conductor of heat, so that the metal near the outside cools rapidly, while that within remains fluid; then if the interior molten portion is drawn off at the bottom, it is found that the metallic shell left behind shows crystalline forms upon its surface. This tendency to crystallize extends throughout the entire mass when it is cooled.

This may be regarded as the first normal condition,* in which there is equilibrium between the attractive forces of the several groups of atoms, and the repulsive forces in the ether envelopes.

If the body has the temperature of the surrounding air, the radial ether vibrations of both are of equal intensity, and the velocities are equal. Every change of temperature, therefore, causes a destruction of the internal equilibrium, and a consequent wave-motion of the groups, causing decomposition into atoms or molecules, with a consequent change of volume, which, if maintained, corresponds to a new normal state. If besides temperature, mechanical forces are acting, such as compression or tension, the wave-motions or disturbances of the groups are either suppressed or hindered; hence the new normal state depends upon the qualitative or quantitative operation of the external forces.

* Two molecules may be so situated that their molecular forces are in equilibrium. Such a condition, due to internal forces only, is called a *normal state.*

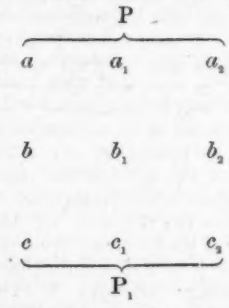
Cast iron, bronze and brass, are generally in the first normal state when taken from the foundry, but not so wrought iron and steel. The latter metals run through a series of normal states under the hammer and roller before they are ready for use.

Experience shows that in the case of iron, which contains only a small per centage of coal, hammering and rolling while hot, when the atoms are in oscillation and the groups are for the most part decomposed, causes a distribution which produces a fibrous grain. This may be explained as follows: The motion of the ether is diminished, or turned in other directions by the hammer or the roller, so that groupings of atoms take place; these groups are brought nearer each other by the working of the metal, until the ether envelopes by virtue of their force of repulsion prevent a further approach of the groups. Suppose the direction of the pressure of two rollers to be vertical, then the vertical dimensions are diminished and the horizontal increased, so that the atom-groups of some of the vertical series are displaced, and push the groups of other series in such directions that no external forces oppose them; and as a consequence new series are formed, so that, for example, groups which were at the corners of cubes assume a pyramidal form.

The square form of a perpendicular section is changed to a lozenge, then to a rectangle. Now, if a body of the last form is broken by slow bending, the horizontal laminæ separate from one another, because the ether in the vertical series is compressed, and the upper horizontal series are stretched. As this extension is not uniform the rupture section has a fibrous grain. Perhaps, large crystals have been resolved into smaller, and these, working in between the larger, give a fibrous look. But if the rod is broken by a sudden blow, the rupture has a crystalline aspect, because the rupture is due to shearing, and the longitudinal fibres have not had time to become extended.

So it is with steel; with the qualification that the form of the molecules and the distribution of groups is different.

It is not possible that working and rolling can force the atoms or molecules of a crystalline group into actual contact; since, in that case the repulsive force of the ether must be done away with, and the attraction of the atoms would become infinite, so that the decomposition of a crystal would be impossible. Hence, we may assume that in every group in the condition under consideration, there is a very dense ether atmosphere, which, when aided by external forces, causes a subdivision into smaller crystals, and then a reduction of these into atoms or molecules brought into close contact, so as to produce an amorphous condition. This decomposition is helped along by the mutual attractions of the exterior atoms of two adjacent crystals, and hindered by the intervening ether. Suppose two opposed bands of external parallel forces uniformly distributed through a very thin plane section, whose resultants are P and P_1, then the external forces will draw away the crystals a, a_1, a_2, and c, c_1, c_2,* from the middle set b, b_1, b_2, be-



cause the repulsive forces of the ether envelopes is aided by the external forces. The squares change into rectangles, and a motion of the easily disturbed ether is induced. It becomes denser in the horizontal intervals $a\,a_1$, $b\,b_1$, &c., than in the vertical, $a\,b$, $b\,c$, &c., so that there is a flow from the latter to the former. But at the same time a radial motion of the ether takes place from a to b and back again; hence the heat phenomena observed by Wohler in cases of great strain. Electric and magnetic phenomena may also occur, since there must be rotatory motion of the ether on account of the greater density about the horizontal diameter of the crystal. In the

* For the easier comprehension of this, let a, b, c, etc., represent cubic crystals, each separable into 8 equal cubes.

first stage the equilibrium of the ether external to the crystal takes place, diminishing density in a horizontal direction so that the attraction of the crystal is less hindered, and the vertical elements $a\ b\ c$, $a_1\ b_1\ c_1$, etc., approach one another diminishing the transverse dimensions in the second stage.

The envelops within the crystal are induced by their repulsive force to take part in the equilibrating movement of the external ether; but are hindered by the attractions of the crystal molecules or atoms, and can therefore express their force only by operating upon the atoms or molecules of the crystal, thereby setting in motion those parts which are applied to adjacent crystals, since these suffer a less pressure on the opposite side. In this way a disintegration is effected of crystals of the first into the second, third, etc., in order so that in the third stage the material is uniformly distributed. These three stages are included in the short interval of time of a single stress within the limits of elasticity. If the forces P and P_1 cease to act, then the original condition recurs: but in our experiments the strains occur in rapid succession, and, only those displaced atoms or molecules which are in close proximity can reunite into crystals of the second or third order. Here we discover the reason that not only the number but the time of duration of stresses has an influence upon rupture.

Perhaps it is not certain that crystalline structure changes to amorphous with every stress, especially with the first; for rods broken after a few strains show a crystalline rupture. But that regular forms become smaller and that the amorphous condition increases, is shown by the smooth mirror-like elliptical spots on the broken surfaces of different kinds of steel. The molten-like spots in iron are to the same effect.

A new normal state occurs at each diminution of crystals, corresponding to a new kind of elasticity; so that instead of a single limit there is a series. But with each change of limit the strength of resistance increases; and the raising of the limit by working and loading is proven by many experiments.

Moll says: "The more crystalline the structure of a body, the less its resistance. The rupture of such bodies is due to the separation of the small crystal groups, not to the breaking up of single crystals. The cohesion of the molecules that form a crystal is greater than that of crystals with one another. The strength of a body increases as its structure approaches the amorphous state when its atoms have a homogeneous distribution."

Though the truth of this statement has been shown by experiment, it will be instructive to investigate the causes. Consider a vertical column of crystals a, b, c, etc, each of mass m; which may be regarded as concentrated at its centre of gravity. Let e, be the common distance between the centers of gravity; and K an unknown co-efficient, then the attraction of b by a, is

$$A_1 = K\,\frac{m^2}{e^2}$$

Suppose each crystal divided into two equal crystals, *e.g.*, a into a' and a'', b into b' and b'', &c., and suppose a' remains in the place of a; that a'' is displaced through one-half of the space $a\,b$, so as to be distant $\frac{1}{2}e$ from a''; then a'' is attracted by a' with the force

$$A_{1/2} = K\,\frac{m^2}{4} \div \frac{e^2}{4};$$

obtained by putting $\frac{1}{2}m$ for m and $\frac{1}{2}e$ for e in the above equation. Of course $A_{1/2} = A_1$; hence the attraction of two adjacent groups has not increased according to the law of gravitation. Taking four groups, a, b, c, d, we have the following results:

b by a, attractive force, $K\frac{m^2}{e^2}$

c by a, attractive force, $K\frac{m^2}{4e^2}$

d by a, attractive force, $K\frac{m^2}{9e^2}$

$$\text{Total, } S_1 = K\frac{m^2}{e^2}\left(1+\frac{1}{4}+\frac{1}{9}\right)$$

Suppose these groups broken up into crystals of half the size a', a''; b' b'', we then have:

a'' by a', attr'tive force, $K\frac{1/4\,m^2}{1/4\,e^2} = K\,\frac{m^2}{e^2}$

b' by a', attr'tive force, $K\frac{1/4\,m^2}{e^2} = K\frac{m^2}{4e^2}$

b'' by a', attr'tive force, $K\frac{1/4\,m^2}{9/4\,e^2}=K\frac{m^2}{9\,e^2}$

c' by a', attr'tive force, $K\frac{1/4\,m^2}{16/4\,e^2}=K\frac{m^2}{16\,e^2}$

c'' by a', attr'tive force, $K\frac{1/4\,m^2}{25/4\,e^2}=K\frac{m^2}{25\,e^2}$

d' by a'. attr'tive force, $K\frac{1/4\,m^2}{36/4\,e^2}=K\frac{m^2}{36\,e^2}$

d'' by a', attr'tive force, $K\frac{1/4\,m^2}{49/4\,e^2}=K\frac{m^2}{49\,e^2}$

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hence $S_2=K\frac{m^2}{e^2}\,[1+1/4+1/9+1/16+1/25+1/36+1/49]$

We find $S_2 > S_1$; and as a, b, c, d are subjected to the same attractive force S_1, while a' is attracted by a'', b' by b'', &c., with the force S_2, it follows that the cohesion of a' with the vertical fibre is greater than that of a. But whatever holds true of one fibre, holds for the sum of all the vertical fibres of a rod; and we may infer that the tensile resistance of a rod increases along with the disintegration of the primitive crystals up to the amorphous condition. This increased resistance might be weakened again if the repulsive force of the ether increased in the same ratio. But the ether envelops of the new crystals become less dense because of the diminished mass of the latter; and the ether in the body is spread throughout a larger space, since the specific gravity of a body diminishes as the volume is increased.

The disintegration of crystals of the first order may also be due to continuous load, when sufficiently small increments of load are added at regular intervals. As soon as resolution of crystals of the first order into the second is effected, permanent extension results; that is, the first limit of elasticity is reached. But as long as only a few crystals have parted from the mother-crystal and are lying in close proximity, it is possible that these may be restored with the removal of the strain, or when it has been reduced to a very small amount. If the crystals of the second order have been changed to the third the second elastic limit is reached, and between this and the first limit the metal is as elastic as before. This explains the raising of the elastic limit under increasing tension.

Knutt Styffe, Director of the Institute of Technology at Stockholm, defines the limit of elasticity as follows: "If a rod of steel or iron is stretched by continual increase of load, at first so small as to cause no sensible permanent extension, then is gradually increased, and allowed to act for a number of minutes depending on the ratio of the increase of weight to that of the whole rod: then the elastic limit is the weight which produces a permanent extension amounting to about the ratio of the increase of weight to the entire load. Let P represent the total load; dP the constant increment of load; L the length of the rod; dL the increase of permanent extension due to P + dP, if this acts for 100 $\frac{d\,P}{P}$ minutes: then the elastic limit corresponds to the weight for which $\frac{d\,L}{L}$ is approximately equal to 0,01 $\frac{d\,P}{P}$. That is, $100\,\frac{d\,L}{L}\cdot\frac{P}{d\,P}$ =1 or nearly 1."

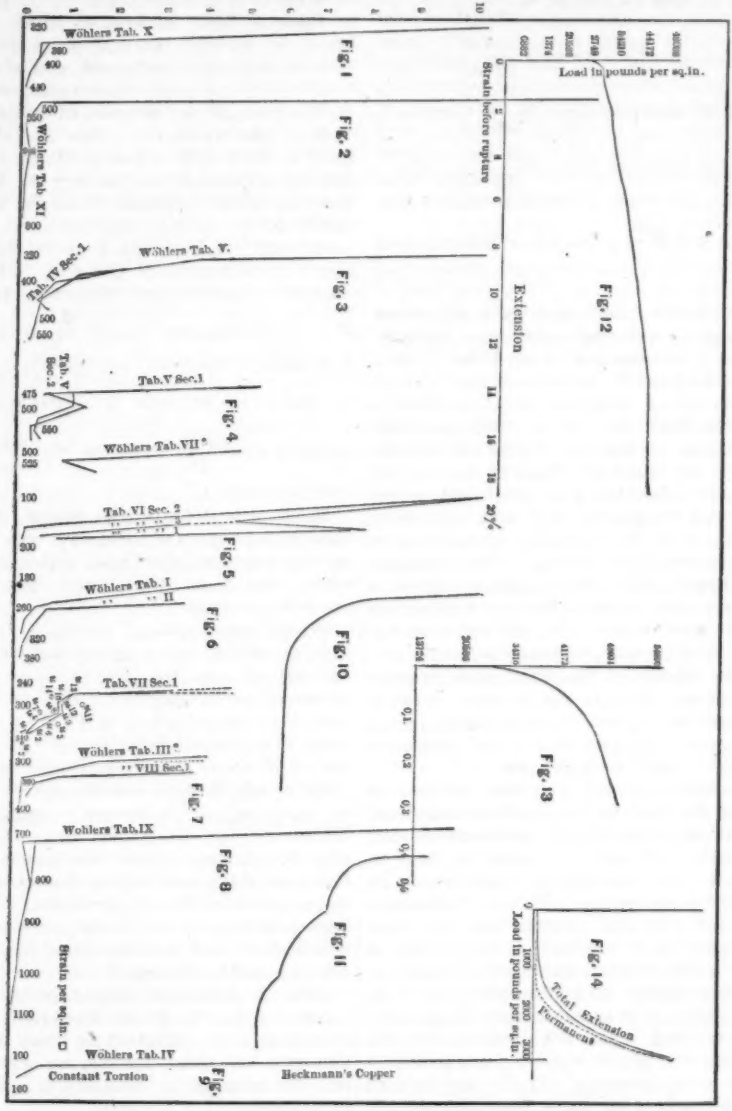
With this definition as datum, Styffe determines in an arbitrary way one of the many elastic limits which lie between the first normal and the final amorphous state.

Styffe gives several curves as examples in which the applied weights are laid off as ordinates, and the per cents. of extension, as abscissæ. One of these curves is represented in Fig. 12. Observe the several elevations of the curve (in which there is an appearance of periodicity) which may correspond to what we have called the several normal conditions. Fig. 13 is the beginning of Fig. 12 to a larger scale: the elastic limit lies near the point where the curve has the smallest radius of curvature. These curves have some similarity to the curves 3, 5 and 6; and possibly some relations, not yet made obvious.

Moll & Reuleaux maintain that the elastic limit may be changed by a change in the normal states; but that in the case of some metals, as wrought-iron for example, its magnitude is slightly affected.

Wohler also found in experiments in bending that the elastic deflection depends not upon the increase of the total deflection, but upon the total load alone. He concludes that

permanent and elastic change of form cannot depend upon the same physical property. This opinion we share with him, for this and other reasons. According to our hypothesis permanent extension is associated with disintegration of



mother-crystals caused by the ether-repulsion. The diminution of transverse dimensions, when a rod is lengthened, is proportionally smaller than the elongation, since under extension the specific gravity diminishes.

Elastic extension is due to the fact that the equilibrium between the attract-

ive forces of the nuclei and the repulsive forces in the envelops is disturbed by the external attractive forces with loss to the former; the disturbance disappearing at the same time with the cessation of the action of external force. Perhaps we may say that the elastic extension depends upon the momentary suppression of the general attraction of the masses (excluding physical and chemical forces); and that it remains constant with equal increments of the strain, because the mass of the rod remains constant.

Fig. 14, shows that the limitation of elastic phenomena to certain metals is a necessary conclusion. The ordinates of the full curve show the total extension in English inches of a bar of phosphor-bronze; the abscissæ correspond to the loads in English pounds; the ordinates of the broken curve represent the permanent extensions. The differences of the ordinates correspond to the elastic extensions which increase from 0, to 22000 lbs., then decrease to 33916 lbs, when rupture took place. Perhaps it happened that after the breaking up of the compound molecules, groups of copper, tin or phosphorus formed, and the groups of the last-named flowed among the other groups so as to prevent the approach of the first two kinds.

The above explanations seem to contradict the well-known phenomenon that extensions generally increase more rapidly as the load approaches the breaking weight. We think that this is true only up to the beginning of the amorphous condition due to repeated or increased load. A body is then perfectly elastic, only so far as this term applies to bodies not absolutely homogeneous. While each increase of load induces a separation of the crystals such that the sectional area does not diminish just as the length increases, no increase of volume can be caused by increase of external forces after the uniform juxtaposition of the atoms. The next addition of tension, if not too great, may cause only a lengthening parallel to its own direction and a contraction of cross-dimensions. But the molecules of the cross-section are brought together as closely as if pressures from without were acting. Hence molecules may be forced out of the cross-rows to form new rows; or new groups are formed, so that the section is permanently diminished: for if tension breaks up groups then pressure must tend to construct them.

Together with the permanent diminution of the section there occurs a permanent increase of length and *vice versa*. Hence results formation and dissolution of groups, with a rapidity proportional to the increase of the force of tension. Finally there comes a load so effective that the groups have not time to break up, and rupture takes place by means of shearing.

Indicate the breaking load per square unit of original section by P_b; this strain, in case of repeated tensions, does not correspond to unlimited duration; but possibly to the smaller value P_n, which is in equilibrium with the amorphous condition. But this can hardly be granted, since P_n as soon as brought into action would cause sudden and various changes of crystals, so as to cause an amorphous condition in some fibres, not in others; so that shearing would be induced and rupture would follow. This view is supported by the fact that iron rods which had borne repeated tensions, when broken by pull were found to be bent, though before rupture they were quite straight. This could not be charged to the excentric application and working of the tension, since in most cases the curvature was opposite in direction to that which might be due to transverse operation of the machine.

It may be shown that the strain corresponding to an unlimited number of tensile strains is $u = \frac{1}{n} P_n$. Since the time of change to the amorphous condition (and therefore n) cannot be determined in the ordinary experiments, it is best to determine u, or the working resistance a by Wohler's method. Wohler's question: "Is there any limit of tension which is perfectly safe?" cannot be answered in the affirmative, since that strain must be sufficient to produce the amorphic condition directly and must not be great enough to form crystals in the cross-section by pressure. The use of iron for girders in buildings need not be rejected; yet it is worth while to consider what may be the effect of the vibrations produced by the continual passing of vehicles.

It hardly needs mention, in order to reconcile an apparent discrepancy in the experiments of Styffe and Sandberg, that if the strain P_1 is applied, rupture must take place by sudden shearing, because the groups have not time to break up and pass over to the elastic limits of new normal states.

The results of the experiments of the former on extensions between $-37.7°$ and $200°$ C. were: (1.) The absolute strength of steel and iron is not diminished by cold; being at least as great at the lowest temperature in Sweden as at the ordinary. (2.) The extensibility of steel and iron is not less at very low temperatures than at ordinary. The fact that car rails and axles break more readily in cold weather, Styffe attributes to the circumstance that the road-bed is less elastic, so that shocks are more intense.

In order to test this explanation, Sandberg, who translated Styffe's work into English, made experiments on steel, the results of which he published in the appendix to his translation. He assumed that the elasticity of granite does not vary between the limits of a hot summer and a cold winter day. He had a granite rock smoothed and leveled and placed upon it two cubical blocks of granite to serve as supports for the rails to be tested. Each rail was divided into two halves; one of which was tested in summer at $+29°$ C.; the other, in winter, at $-12°$ C., by dropping upon them a sphere weighing nearly nine centners. The sections of ten bars showed that at the lower temperature iron had but one-third or one-fourth of the resistance which it had at the higher; also that the tenacity and resistance to bending were notably affected by cold.

Regarding these results as trustworthy, we explain them as follows: The radial motion of ether which appears in the form of heat, after some time causes the same disintegration of crystals as repeated or increased strains. But cold, like compression, which is attended with less vibration, promotes the formation of groups; these diminish tensile resistance, and make the material brittle on account of its crystalline structure. Sandberg subjected the rails, which had become crystalline under the influence of cold, to a sudden blow; of course they broke more readily in winter than in the summer. On the other hand, Styffe broke his rods with a hydraulic machine; increasing the strain gradually, so that he gradually broke up the connection of the crystals, thus substituting mechanical work for ethereal vibrations.

As fibers are not homogeneous in their structure, they must contain a varying number of crystals. Those fibers in which the crystals are not so dense, are more strained by tension so that the groups are quickly broken up. Forced by the ether they leave the molecular groups of fuller fibers and seek the fiber most strained in order to restore equilibrium. Hence the ray-like appearance of the fracture-surfaces of steel and phosphor bronze. The fibers nearest the weakest fiber are more extended, and are successively brought beyond the amorphous condition so as to have less tenacity. The effective section of resistance becomes gradually smaller; and at last the dynanometer breaks the remaining crystalline section, which therefore has an appearance as if sawed or broken with the hammer.

That bars of square section break at the corners is explained by supposing that the flow of molecules to those parts is less free than to others. In the broken steel rods there appeared on the upper edge a small plane (wedge) inclined $45°$, having the appearance of being pushed out. This is the angle of maximum shearing effect.

Crushing proper cannot be produced by pressure when it causes approach of molecules and formation of crystals. Separation can be caused only by shearing, or by "nicking," or by the increase of sectional area, from the ends toward the middle, causing a deficiency of molecules at the edge. The last may be the reason of the sudden fracture of bars subjected to alternating tension and compression. Wohler says: "Pieces bearing positive and negative strains must be made stronger in the ratio 9 : 5 than those strained only in one direction." The explanation of this is not easy. Launhardt's is not sufficient, for he considers the case in which the limits of elasticity are passed on both sides. Wohler's hypothesis that the difference of strains suffices for rupture, and that in the case of alternating tension and

compression this algebraic difference is the sum of the numerical values of the strains, is also insufficient.

$$\begin{array}{cccc} A & & B & C \\ \hline A & & B & D \\ \hline A & C_1 & B & C \end{array}$$

It is probable that if a fibre A B is stretched longer by the amount BC, there is less molecular change than if stretched by a length B D longer than B C; but in our opinion it does not follow that B C + B C′ produces the same change as B D, if C C′ = B D. We should rather believe that if the elongation B C were somehow suppressed, the fibre would return to its normal condition, and that the shortening .B C′ could not be more injurious than if it occurred in the normal state without previous elongation. The only satisfactory explanation seems to be involved in the theory of Moll and Reuleaux that, for the same normal state, there are two co-existent limits of elasticity, one for tension, the other for compression.

If the elastic limit of tension is raised by repeated strain, *i. e.*, if attraction is increased and repulsion diminished, then the elastic limit of compression is diminished. Hence, if the elongation B C due to tension lies within the elastic limit, the equal compression B C′ causes a permanent shortening, and after some time a rupture. But it is also possible that compression causes a bending toward one side or an elongation of sets of fibers near the periphery, so that a new normal state results in that part, which reduces the section of resistance. The case of a rod under repeated torsion is not relevant to a decision of this question, because each fiber is alternately convex and concave. Other experiments must be made with new machines adapted to changing axial or transversal positive strain into negative. We may then ascertain what is the effect of a greater strain of one kind following a less of the other. We think it will be found that our assumption is not far wrong, that a slight compression following tension is rather advantageous than the contrary.

Wohler's observation that between + 440 and + 240 Ctr., and between + 300 and 0 vibrations may occur with equal safety, requires the confirmation of further experiment.

In consequence of the construction of the machines for bending and pull the dynanometer must first give a strain of 240 Ctr. in the outermost fibres; then the extreme strain of 440 Ctr. is applied; hence a normal state must result different from and higher than that which would follow the tension zero. It may be asked what would be the normal state if the strain 440 were first applied, and then the strain 240. Perhaps the result would be the same as if the force P_b spoken of above should act. The answer may have a practical bearing with reference to bridge building. The weight proper of structure corresponds to the lower limit 240 Ctr., and if the rolling load increases the strain by 200 Ctr. the total permissible strain is reached. Suppose the weight of structure uniformly distributed, then the vertical component is a maximum at the ends and zero in the middle, and the tension members at the abutments are in better condition to bear external forces than those in the middle, on account of the raising of the limit of elasticity. The former may therefore for a total load of 440 Ctr. have "indefinite duration," because they may rise from 240 to 440 Ctr. tension; but not so of the latter, since they pass from 0 to 440 Ctr. strain; 300 being the limit. Assuming 110 Ctr. as the permissible strain, we should have 4-fold security at the abutments, but less than 3-fold in the middle.

We shall now attempt to account for the appearence of the surfaces of fracture of rods broken by repeated torsion. In the case of steel rods the circular section-surface was divided into two unequal segments; one of which, that next the side of initial fracture, and the smaller, was close grained and smooth; the other being coarse-grained and somewhat crystalline in appearance. This seems to contradict the statement above made that rupture takes place only on the tension side and that the adjacent surfaces pass over to the amorphous condition; for each fibre of these rods was pulled during half a rotation and compressed during the other half. The process may be as follows:

If the rupture at a is the consequence of tension, the bending of the rod, since the most efficient fibre has ceased to resist, will be greater than if it had turned through an angle $\propto$ bringing a to a_1 or to a_2 (the neutral section) while an unbroken fibre is brought to the top. In its further progress to a_2 or from a_1 to a_2 the broken fibre induces no greater

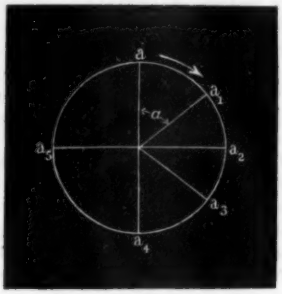


FIG. 6.

bending since the broken parts support each other. Indeed it is possible that the bending is less during the last half rotation than during the first. If before fracture there has been a permanent extension of the fibre a and the adjacent fibres, the rod is permanently bent downward. After a half rotation it would be bent upwards, were it not for the fact that the stress of the dynanometer acts downward. But in no case is the upper fibre so much bent as it would be if the elastic limit of the lower fibres had been passed.

It would now seem to be clear that if the fibre a is at the top, the adjacent fibres reach the maximum stress, causing the amorphous condition of the upper segment. Upon the side opposite a the crystals are larger than they are near the chord of division, showing a higher state of disintegration.

Appearances similar to those of steel are observed in iron, but they are not so sharply defined.

If the equilibrium of the ether and the consequent amorphizing is in any way hindered, there may be other segments of rupture.

The rupture of iron and steel under torsion shows a smooth surface adjacent to the fracture, while that of bronze is shining, and phosphor-bronze is often dark-brown or blackish on the opposite side. This may be explained by supposing that after the breaking up of crystals into molecules, these partly resolve into atoms, those of tin gathering at one place, those of copper and phosphorus at another.

We must reject the theory that iron is made crystalline by repeated tension, since bending and tension appear, by our experiments, to break up the crystalline structure; while compression promotes it. So the upper parts of rails become crystalline and hard because subjected to compression. Whether irregular shocks promote crystallization remains to be determined by experiment.

The rupture-sections generally show in what way breaking has occurred; whether by repeated strains or by the sudden shock of a great load. This may be of import in discovering the cause of an accident, as, for example, whether it is due to a broken axle or a broken rail. This might be determined by an examination of the peculiarities of the sections of fracture.

There remains to consider the possible effect of heat in impairing strength by vibration of the molecules. Heat separates the crystals, causing change of position in all directions: and when it is uniformly distributed, if the amorphous state occurs, the strains in cross-section are exactly equal to those in longitudinal, so that there is either diminution of section, or re-grouping only, which is not competent to effect rupture as long as the aggregate state is unchanged.

We have had Krupp's axle steel hardened and then broken; the grain was much closer and smoother than in the same not hardened. But if steel is first heated and then hardened the rupture-surface becomes more like that of the non-hardened, according to the duration of the heating; while the grain of steel heated and not hardened is coarse and globular. So is that of heated iron. Hence it would seem that red-heat produces no tendency to form groups, while a higher heat, long continued, induces a storm of ether atoms, so that in cooling, they gather in irregular groups; a phenomenon observed in cast metal.

The experiments of Styffe mentioned above, which show that the absolute resistance of steel and iron at high tem-

peratures, up to 200° C., does not diminish, while that of soft iron increases, contradict Muller, in the Zeitschr. des Ost., Ing. u. Arch. ver. (1873) where he says: "When a bridge-member, in the course of twenty-four hours, suffers a lowering of temperature to the amount of 15° C., there is a change of its length amounting to 0,00022. The elastic change of length is the same up to a load of one-third the limit. So a change of temperature has the same effect as a load."

Finally, we refer to the diagrams. In all the polygons there is a point from which the ordinates increase very rapidly. This may be compared with that one in Styffe's curve, Fig. 13, which corresponds to the shortest radius of curvature, in the vicinity of which lies the limit of elasticity. We may conjecture that the abscissa corresponding to this point, *i. e.*, the load, answers to some important condition of the metal, possibly to that in which the irregular crystalline structure becomes more regular. It may be that, up to this point, the number of strains necessary for rupture depends upon the specific nature of the rod, while it afterwards depends upon the general properties of the metal. Hence it may happen that irregularities of the polygons are more frequent before this point is reached. That non-homogeneous metals may be improved by repeated strains is not impossible. Suppose, for example, there is a thin section of slag running across a rod. This may be broken up into several parts by small tensions, which may be disposed by the motion of the ether into a direction parallel to the tension, so that the decomposition of crystals is less hindered. The attraction of homogeneous molecules to both sides of the section may be diminished, but not destroyed; and there may be an equilibrium between the force of attraction and the repulsive force of the ether increased by the action of external forces. A greater external force would break the rod at this section.

The less obvious this point in the polygon the more homogeneous and pure the metal. This appears in Fig. 9. And, from Fig. 7 it may be inferred that Krupp's steel is now much more uniform and stronger than that of 1862.

Time and space forbid further presentation of facts supporting our hypothesis. It is to be hoped that others will pursue the subject further.

The following corrections should be made in the first portion of this article in the last number:

Page 450, last line, read 45,000,000.

Page 452, 2d line from bottom, 2d col. read "to have much greater strength than."

Page 459, 22d line from bottom, 2d col., read "occurred after 10 million."

Page 459, 4th from bottom, 2d col., read 800 instead of 500.

Page 460, second from top, 1st col. read 480 instead of 450, and 350 instead of 300.

Page 460, 9th from top, 400 Ctr. instead of 900 Ctr.

COHESION AND CRUSHING.

From "The Engineer."

THE resistance which a body offers to strain, whether tensile or compressive, is clearly due to the cohesion of its particles; that is, to the force which sticks or binds them together. The proper mode of ascertaining the behavior of a body under strain would therefore be to deduce it from the laws of cohesion. Unfortunately, of these laws we at present know nothing. There is no domain of science in which so little progress seems to be made as in this. Whilst we know with the utmost exactness how it is that the earth is held in its constant orbit round the sun, we are completely ignorant of the forces which hold in one mass the stone which lies at our foot. It has been said that no future philosopher can ever hope to rival the fame of Newton, because the law which governs the universe can be discovered but once. But the laws of cohesion hold just as widely, and perhaps are of even greater practical importance. There is ample

room for a second Newton to take rank with the first by proclaiming their discovery, and when a second Newton arises, perhaps the discovery may be made. Meanwhile, all we can do is to feel our way by the light of such principles as, in the general darkness of the subject, we can still manage to discern. Let us consider what are the elementary facts which may be observed when a body (for instance, a bar of iron) is subjected to longitudinal strain. Of these the most obvious is that the body alters its dimensions, becoming longer if the strain is tensile, shorter if the strain is compressive. In other words, the particles of the body follow the direction of the strain, becoming further apart if it tends to separate them (tensile strain), and nearer together if it tends to unite them (compressive strain). The particles oppose a powerful resistance to this motion in both cases, and it is this resistance which, gradually increasing until it equals the external strain, brings the body once more to rest. Now, this resistance must be due to a force of repulsion existing when the particles are urged towards each other, and a force of attraction existing when they are drawn apart. It follows then that the force of cohesion acting between these two particles must be twofold, consisting of an attraction and a repulsion. These must alike be insensible at any except very small distances, so small as to be quite beyond all ordinary modes of measurement. This is evident from the fact that two bodies when placed in contact do not cohere, nor can they in general be made to do so by pressure, however powerful. The only explanation of this is that, owing to the roughness of the surfaces, the number of particles brought sufficiently close to act upon one another is exceedingly small, and therefore the resistance they oppose to separation is but trifling. Cases are however recorded in which surfaces of metal have been brought to so high a degree of polish as to cohere together with considerable force. This is generally attributed to the pressure of the atmosphere, the surfaces being supposed to be so smooth that no air can find its way in between them. This is in itself sufficiently improbable, whilst it must be remembered that this external pressure would oppose no resistance to the sliding of the surfaces over each other, but only to their being drawn directly apart. There would thus be no real adhesion in the ordinary sense of the word. It would seem much more likely that in these cases the inequalities of the surfaces have been so far planed down that the forces of cohesion do to some extent come into play. Be this as it may, the fact remains that these forces only act at distances, not infinitely, but immeasurably small. It is not meant by this that the forces absolutely cease to exist at greater distances. This would not be impossible, but would involve complicated laws of force which it is not necessary to assume. The conditions are satisfied by supposing that they vary inversely according to some power of the distance. This is actually the case with regard to one force which is known to act between particles, namely, the force which we call gravity. The law of gravity is expressed by saying that every particle in the universe attracts every other particle with a force varying inversely as the square of the distance between them. In other words, if we represent by 1 the amount of the force at a distance of 1 in., then at 2 in. it will be represented by $\frac{1}{4}$, at 3 in. by $\frac{1}{9}$, and so on. It is evident how rapid is the degradation of force under such a condition. As a matter of fact, we know that the attraction between two bodies, even when in contact, is quite inappreciable. Even the enormous mass of the earth only exercises on a body at its surface that moderate force to which we give the name of weight. But if we suppose two particles whose distance apart is immeasurably small, the case is quite different. The force would then become considerable; and, in fact, if we could imagine the two particles in actual contact, the attraction would become infinite, or no finite force would be able to separate them. It would seem, then, that gravity is exactly the kind of attractive force whose existence we have been proving from the phenomena of cohesion. Hence, as we know that gravity exists, it seems just to assume that this cohesive force of attraction is nothing else than gravity acting at small distances. With it, as shown above, must be combined a repulsive force of similar

character. This repulsive force is greater than the attractive within the distance of equilibrium, as is shown by the fact that force is necessary to produce compression; but beyond that distance it falls rapidly below the attractive force, so that in the motions of the heavenly bodies no trace of it is visible. This will be accounted for most simply by supposing the force to vary inversely according to some power of the distance greater than the square. On this supposition the algebraical expression for the two forces together would be $\frac{k}{d^n} - \frac{k^1}{d^m}$ where d is the distance, k k^1 are constants, and m in some quantity greater than 2.

Of course, this expression is only to be accepted as an hypothesis; nor is it offered as fully representing the apparently complicated forces which produce the phenomena of cohesion. As far, however, as the results due to strain are concerned, it seems sufficient to answer our purpose. It follows that, in considering the effects of strain on any particle of a body, all the other particles except those immediately adjacent may be left out of consideration. In fact, the particle may be considered as occupying the center of an exceedingly small solid sphere, the constituent particles of which exert upon it a force of attraction or repulsion according to their respective distances. It is thus that the forces of cohesion have been looked at by a French engineer, M. Pelletieau, in a paper published in the *Annales des Ponts et Chaussees* for July, 1873. From thence he draws several conclusions with respect to the behavior of materials under a compressive or crushing strain, which, if true, are of great importance, but which appear to the writer to be open to considerable criticism.

It may be well in the first instance to state what these practical rules or conclusions are. They apply to a prism of any material, standing upon a plane base and crushed by weights placed upon it. They are as follows, all other things being supposed in each case to be equal: —(1) The resistance to crushing is greater, as the friction between the prism and the plane is greater; and is greatest when the prism is absolutely fixed to the plane; (2) the resistance to crushing is greater as the base is more flexible, or in other words, yields to a greater extent under the load; (3) the strongest form that can be given to the prism is that of a circle; of polygonal forms that which approaches nearest to the circle, or has the largest angles, will be the strongest; (4) the crushing strain will be greater proportionally for a small prism than for a large one; (5) the crushing strain will be greater as the height is less. Before discussing these rules we must explain what is here meant by the crushing of the prism. There are several ways in which bodies may yield under compressive strain. In Rankine's *Applied Mechanics*, art. 285, there are enumerated five, viz.: (1) Crushing by splitting into prismatic fragments; (2) crushing by shearing, or sliding of one portion upon another along oblique surfaces of separation; (3) crushing by bulging or lateral swelling and spreading of the prism, which is the usual form with ductile materials, such as wrought iron; (4) crushing by buckling or crippling, as in wood and other fibrous materials: (5) crushing by cross-breaking, which happens with very long pillars only. It will be observed that these are all what may be termed indirect methods of yielding. There can, in fact, be no such thing as direct crushing of a body, analogous to its breaking under a tensile strain. The pressing together of two particles can only produce an increased resistance due to their mutual repulsion; and even an infinite compressive force could do no more than bring them into actual contact. In fact the breaking of a body, whether by crushing or tearing, can only be due to separation, not compression of its parts; and this is in fact what happens in all the five cases enumerated above. Of these, the second and fifth are due to peculiar causes; but the other three are clearly modifications of the same result, namely, the splitting or tearing apart laterally of the prism under the distension produced by the compressive strain. The fact of this distension or lateral spreading of the body under compression is certain, and may be shown by experiments on any highly elastic substance. Thus, if a short block of india-rubber, be squeezed between the finger and thumb it will be seen to thicken out most perceptibly, at the same time that it short-

ens under the pressure. The cause of this lateral thickening is not so obvious; but, in fact, it may be seen to follow from the principles of cohesion explained above, and thus affords an additional verification of those principles. Thus, let us imagine a body composed of six particles only, *a*, *b*, *c*, *d*, *e*, *f*, which are

in equilibrium under their respective attractions and repulsions. Now, suppose a compressive force to act upon the body, and to bring the particles *a*, *b*, *c*, into the position a^1, b^1, c^1. Let us then consider what will happen to the particle *d*, and whether it will have any tendency to move laterally, this is, along the line *d e f*. As the particle was originally at rest, the forces tending to move it along this line must have been in equilibrium. These forces were repulsions due to those of the adjacent particles which were within the distance of equilibrium (or that at which the forces of attraction and repulsion are equal) and attractions due to those which were beyond it. Let us suppose the forces to have been in fact a repulsion due to the particle *e*, and attractions due to the particles *b*, *c*, *f*. The particle *a* will of course exercise no effect in the lateral direction. Now, when the compression has taken place it is evident that the distance of *d* from *b*, *c* (in their new positions b^1 c^1) has diminished. Consequently, the attraction of those particles has diminished also, for this attraction is zero at the distance of equilibrium, and increases thenceforward (the attraction here spoken of being, in fact, the difference between the force of attraction and the force of repulsion at that particular distance). But while the attraction of *b c* has diminished, the forces due to *e f* remain the same; hence, as there was equilibrium before, there will now be an unbalanced force of repulsion due to *e*, and therefore the particle *d* will move in the direction *e d* until, by the increase of distance, this unbalanced repulsion is diminished to zero. By exactly similar reasoning it may be shown that *f* will move along the line *e f*, and that *a c* will also move outwards; *b e* will alone retain their positions, being balanced by the forces on either side of them. It seems clear that similar effects will be produced where the number of particles is very great, as in ordinary cases, and thus the distention or spreading outwards of a body under compression is satisfactorily accounted for. The contraction of area under tensile strain, which is so familiar a feature in experiments upon extension, may of course be accounted for on precisely similar principles. It is not, however, so easy to see how this distension of the body under compressive strain can ever result in fracture, as the repulsive forces to which it is due continually diminish in intensity as the distance between the particles increases. In fact, it seems probable that in a perfectly homogeneous body no disruption would ever take place, and it would squeeze out under the pressure as an inelastic substance does. But as a matter of fact, no bodies are perfectly homogeneous. There always exist in them weak points, which we may conceive as *lacunæ* or crevices, partly bridged over by particles hanging in suspense, as it were, between the attractions of the two walls. Now, if an unequal pressure be brought upon the two sides of this crevice, thus modifying in the way described above the attractions which they exercise, this state of suspense will cease to exist; the intervening particles will move towards one sidé or the other, and may, if the difference of attraction be sufficiently great, break off permanently from the further and attach themselves to the nearer. Thus, a disruption may begin which will soon propagate itself through the mass. As the general motion produced is outwards, it seems clear that any force acting inwards, or tending to keep the particles of the body together, will counteract the effect of the pressure and increase the resistance of the body. And if there is any line within the cross section of the body along which this inward force is exceptionally weak, then it is along this line that a disruption of the kind described will first take place.

It is thus that the problem of resistance to crushing appears to have present-

ed itself to M. Pelletieau, although he has not given any general view of the subject such as has been sketched out above. The lateral splitting of the prism by distension is what he understands by the word crushing; and his investigation of the most important point—namely, the proper shape of the prism—depends on the last principle laid down, namely, that crushing will first occur along the line in which the inward force, tending to hold the prism together laterally, is least. Such an inward force he shows to be produced by the attraction excited on the particles at the surface. Thus, let M, Fig. 1, be any

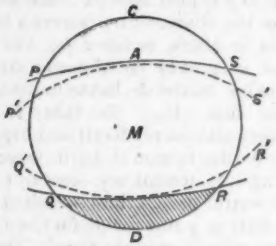


FIG. 1.

particle of a body, and describe round it a circle P Q R S with the "radius of action," that is, the radius beyond which the effect of cohesion may be considered insignificant. Then, if we suppose M to lie in the interior of the body, all the particles within this circle will indeed act upon M, but their respective actions will balance each other, and M will have no tendency to move in one direction more than in another. The case is different if we suppose M to lie so near the surface of the body that this surface comes within the distance of the radius of action. Thus, suppose the circle P Q R S to be normal to this surface, and to cut it in the curve P A Q S. Suppose Q B R to be a line on the other side of M, opposite to and exactly similar to P A S, then it is evident that the particles within the space enclosed by the lines P A S, Q B R, will be symmetrical about M, and the forces they exert will be in equilibrium. But the particles in the space Q B R D will be unbalanced, as the opposite space P A S C is outside the surface. Thus, if we suppose P A S C to have existed originally and to be suddenly cut away, then Q B R D will at once exercise an unbalanced attraction upon M, which will therefore move towards it until it is checked by the repulsion of the particles between M and the line Q B R. The nearer M is to the surface the larger this space Q B R will be, and therefore the more powerful the force with which M tends to move towards the interior of the body. The general effect is, that the outside layer of a body always acts strongly to compress the interior, and may be compared to an elastic envelope tightly strained over it and squeezing it together. It thus resists the distension which is caused by compressive strains, and which according to the views expressed above, produces the crushing of the body. Granting this, will the amount of this resistance vary according to the shape of the cross section, so that with some bodies crushing will take place earlier than with others of the same substance and area but of different form? M. Pelletieau answers this in the affirmative: The principle he affirms is, that crushing will begin at the point of the outline of the cross section at which the radius of curvature is least. Thus, the circle is the strongest form of section, since any other curved figure of equal area must have a smaller radius of curvature at some point. Similarly a curved section will always be stronger than an angular one. M. Pelletieau deduces this result from the consideration of Fig. 2, in which M M' are two particles

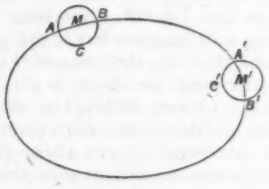


FIG. 2.

situated at the edge of an oval figure, and A C B, A' C' B' are the circles of action drawn round them. Now at M, where the radius of curvature is greater, it is clear that the part A C B of the circle of action falling within the figure is greater. Hence M. Pelletieau concludes that the attraction which this part exercises on the particle will be greater owing to the increase of area,

and therefore the resistance to crushing which that attraction produces will be greater also. But M. Pelletieau has not taken into account the fact that this holds true only for the extreme outside particles of the section. Thus, referring back to Fig. 1, we see that if for the curve P A S, which represents the edge of the body, we substitute another of greater curvature $P^1 A S^1$, then the unbalanced part Q B R^1 within the circle of action is greater, and not less than it was before, and the attraction which it exerts will be greater also. It is true as M. Pelletieau points out, that the whole attraction upon M is less than if it were close to the edge at A, but this does not apply to the difference between the attractions in the two cases, which is what we are concerned with here. It is only when M is so close to the surface that the two curves P A S, Q B R, cut each other some distance within the circle of action, that the opposite effect is produced. It might thus seem probable that the resistance to crushing, instead of being diminished, is increased, by diminishing the radius of curvature. In point of fact, however, all such variations may be neglected on account of the excessive smallness of the circle of action as compared with the circle of curvature, whatever the size of the latter may be. This in all cases makes P A S so very nearly a straight line that the differences of area cut off by it in various cases are insignificant compared with the whole area of the circle of action. Thus the truth seems to be that the resistance to crushing will practically be the same for any form of cross section, provided it be bounded by curves. In the case of sharp angles it is more difficult to come to a decision. The opposing lines P A S, Q B R, in Fig. 1, will then cut each other within the circle of action much sooner than before, and therefore the attraction will decrease with the angle (M. Pelletieau's principle) in particles further within the surface, than where the boundary is a curve; but on the other hand, the unbalanced attraction on a particle some way within the surface, such as M in Fig. 1, will be larger. Putting one of these against the other, as without further knowlege of the laws of cohesion it would be impossible to make an exact calculation, we may consider that in the case of an angular, as in that of a curved section, the form will have no appreciable influence on the crushing strain.

The third and perhaps most important of M. Pelletieau's rules, as stated above, must therefore be held to be incorrect. It remains to consider in order the other four. The first asserts that the resistance to crushing will be greater as the friction between the prism and the plane on which it stands is greater. Thus, supposing the plane to be perfectly smooth, the prism will begin to crush at its base, both because the base has to support the weight of the prism itself as well as the superimposed load, and also because the distension produced by compression is there resisted by the forces on one side only of the section, since there is no cohesion between the plane and the base. If, on the other hand, the base were absolutely fixed and embedded in the plane, it would be impossible for crushing to commence there, and the prism would bulge and crush at some intermediate point between the top and bottom, which would clearly require a considerably larger compressive force. Where the prism is not fixed to the plane, but there is friction between them, intermediate effects will be produced, the prism will still crush at the base, but will require a greater force to do so, since the distension will be to some extent resisted by the friction of the plane.

M. Pelletieau's second rule is that the resistance is greater as the plane is more flexible, that is, yields more under the pressure. He bases this on the supposition that under the pressure the plane will deform into a curve, as shown in Fig. 3, and that the reactions between

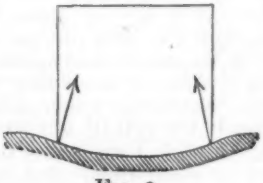


FIG. 3.

this and the base will be normal to this curve, and therefore incline inwards round the edge of the prism, as shown by the arrows. The inward action of

these reactions he considers would resist the distension of the base due to the compressive force, and the greater the deflection of the plane, and therefore the inclination of the normals to the vertical, the greater this inward action will be. But this conclusion would seem to be incorrect for two reasons. In the first place, the inclination of the normals to the vertical would in all practical cases be so small that the resulting inward action could not but be inconsiderable. And secondly, that the action between two surfaces is normal to them, is by no means a universal rule, and in any case only holds good with regard to perfectly smooth surfaces, which these are not. The flexibility of the plane cannot, therefore, be considered to have any effect on the resistance to crushing, provided, of course, that it is not so soft as to crush itself before the prism does so.

The fourth rule—viz, that the resistance will be greater as the absolute size of section is smaller—is founded on the same principle as the rule last considered, and therefore falls with it. It is obvious that with very small sections the reverse will hold, as the narrower the prism the more liable it is to yield by cross breaking.

M. Pelletieau's fifth and last rule is, that the crushing strain is less as the height is greater. He cannot be supposed to mean that the strain varies in proportion, so that a prism 1 ft. high would only bear half the load of one whose height was 6 in.; for this would be against all experience. All that can be understood is, that the crushing strain will be to some extent less in the former case than in the latter. Even this does not agree with the principle generally assumed, which is that the crushing strain is independent of the height, unless the latter be so great that the prism becomes what is called a "long pillar," and is liable to yield by cross breaking. It is probable that this is not absolutely correct, and that even with short pillars the resistance to crushing is to some extent affected by the height. Mr. Eaton Hodgkinson found this indeed to be the case with substances which yielded by shearing, or the sliding of one part over another; but this is not the kind of crushing considered here. No sufficient experiments appear to have been made, at least in England, upon this point; and all that can be said at present is, therefore, that when the height is considerable—say more than five times the thickness—it is as well to take a somewhat enlarged factor of safety.

It appears, then, that M. Pelletieau's five rules, as given above, cannot be accepted as correct. On the contrary, neither the flexibility of the base, the form of the cross section, or the absolute size of prism, can be supposed to exercise any effect on the crushing strain. It is true that this may be expected to be larger as the friction of the base is greater, and also as the height of the prism is less; but in neither case is the variation large enough to be of much practical importance. Hence, the ordinary rule, which makes the resistance to crushing, in any given material, proportionally simply to the area of the block, may still be accepted as substantially true, at least until some exhaustive series of experiments or some more searching analysis shall prove the necessity of its modification.

REPORTS OF ENGINEERING SOCIETIES.

AMERICAN SOCIETY OF CIVIL ENGINEERS.—A communication from George W. R. Bayley, of New Orleans, dated March 31st, enclosing profile of South Pass bar, showing progress, to that date, of improvement at the mouth of the Mississippi, and one from James B. Francis, of Lowell, Mass., of the same date, referring to the failure of the dam of the Leicester reservoir of the Worcester Water Works, were read. Sketches accompanying the latter were examined, and an account of the failure was given, by John T. Fanning, of Manchester, N. H.

Discussion of "Notes and Suggestions on the Croton Water Works and Supply for the Future," was taken up. A communication from James B. Francis, dated April 3d, and one from J. Herbert Shedd, Providence, R. I., dated April 4th, were read; the discussion was continued by Francis Collingwood, J. James R. Croes, Benjamin S. Church, Alfred W. Craven, Julius W. Adams and George S. Greene.

Robert H. Thurston made some remarks upon the peculiarities of the two classes of metals referred to, in a "Note on the Strength of Materials as affected by Time," presented to the Society; and illustrated them by exhibiting typical strain diagrams, obtained by plotting the results of experiments on transverse strength, which were in progress at the Mechanical Laboratory of the Stevens Institute of Technology.

A bar of each class was represented, one of

which exhibited the exaltation of the elastic limit under strain, while the other exhibited depression of the elastic limit and flow. A third bar was very elastic and brittle, and exhibited both flow and the depression of the limit of elasticity.

He next took up the subject of railway traction, and referred to the Dudley dynograph as a means of determining the same, illustrating his remarks by exhibition of the graphic record obtained by use of this instrument.

After consideration of the two subjects, the following were offered—and under the By-Law adopted January 5th, referred to the Board of Direction:

Resolved, that a committee of three be appointed to examine and report upon the recent failure of the dam at Worcester, Mass., the same to be without expense to the Society.

Resolved, that a committee of five (*named in resolution*) be appointed to examine and report upon the resistance of railway trains; such to be without expense to the Society.

April 19th, 1876.—A stated meeting was held at 8 o'clock P. M.

Robert L. Cooke, C. E., of New York, read a paper, giving the "History of the Lehigh Coal and Navigation Company, and its Works."

The discussion of "Notes and Suggestions on the Croton Water Works and Supply for the Future," was continued from the last meeting; communications from William R. Hutton, dated April 6th, and William J. Mc Alpine, dated April 18th, were read, and further discussion was postponed to May 3d.

CIVIL ENGINEERS' CLUB OF THE NORTHWEST. —REPORT OF APRIL MEETING.—The 65th regular meeting of the Club was held at the Sherman House Club rooms on Tuesday the 4th inst., beginning at 4 o'clock P. M; President Chesbrough in the chair and a large attendance of members, including several non-residents. After the approval of the minutes of the last meeting the following persons were elected members: A Case, 38 Portland Block, Chicago; G. F. Kirby, 420 Fifth Avenue, Chicago; C. J. Roney, Chicago University; Henry Stewart, associate editor *American Agriculturist*, New York City; W H. Newton, Chicago. Mr. Greeley read a partial report of the Committee on the Adoption of the Metric System of Weights and Measures, which was accepted and the Committee continued. By request of the President R. J. McClure, C. E., of the C. B. & Q. Ry. was added to the Committee, and on motion it was resolved that the next regular meeting be set apart for the discussion of the Metric System. Papers by C. J. Quetil, C. E., of Camden, N. J., and by W. Sooy Smith, C. E., of Maywood, Ills., were read, and that of Mr. Quetil on the "One Rail" system of railway was discussed by Chas. Paine, Past President, President Chesbrough, the Secretary, and other members.

IRON AND STEEL NOTES.

RAILWAY IRON TRADE OF GREAT BRITAIN.—The additional experience which each month supplies as to the course of our export iron trade this year cannot be said to be favorable. Thus, our exports of railway iron have been as follows in the first three months of the last three years:

Month.	1874.	1875.	1876.
January..Tons	46,598	36,171	23,580
February.....	49,713	35,086	18,099
March	62,992	31,369	21,939
Total.....	159,303	102,626	63,618

It will be seen that the total supplied by the first quarter of this year was not half so large as that for the first quarter of 1874, and that our foreign iron trade relations are just now distinguished by what an Irish statistician once described as a "progressive decline of prosperity." The collapse in the demand for our railway iron in the United States is now almost complete, and it is certainly disheartening in the last degree. In the three months ending March 31 this year we only sent the Americans 85 tons, while our corresponding exports in the same direction, in the corresponding period of 1875 were 9013 tons; and, in the corresponding period of 1874, 28,025 tons. The colonial demand has attained during the last year or two a compensatory importance, but even this resource appears to have failed our industrials this year to some extent. Thus, the exports of our railway iron to British America, British India, and Australia have been as follows during the first quarters of 1876, 1875, and 1874.

	1874.	1875.	1876.
British America..Tons	374	6,221	2,083
British India.....	13,172	10,015	11,497
Australia.....	20,051	22,583	8,245
Total.....	33,597	38,819	21,825

We have been sending a little more railway iron this year to Sweden and Norway, as well as to Spain and Italy; but the collapse of South American credit has had the effect of materially contracting our deliveries of rails to Brazil, Peru, and Chili. These deliveries have moved on as follows to March 31 in each of the last three years:

Country.	1874.	1875.	1876.
Brazil. . Tons	5,796	2,678	3,990
Peru.....	2,008	9,832	1,644
Chili.....	4,170	5,650	631
Total.....	11,974	18,160	6,265

In fact, in almost whatever direction we turn we find our export railway iron trade a prey at present to depression and gloom. What with a reduction in the aggregate amount exported, and the great decline which has taken place in the price of rails during the last few months, the aggregate value of our railway iron exports in the first quarter of this year did not exceed £576,904, as compared with £1,057,135 in the corresponding quarter of 1875, and £2,133,514 in the corresponding quarter of 1874.

It appears to us, however, that although the reduction in the price of rails may cause our iron trade statistics to present a discouraging appearance this year, it contains important elements of future hope. Thus, even assuming

that the American market is hopelessly lost—and if it is so lost, the fact is undoubtedly a great misfortune to the British iron trade—the apparent possibility of once more delivering English rails upon cheap terms and conditions must be a highly important condition of future prosperity. If we can send our rails at a cheap rate to British India, Canada and Australia, the authorities and capitalists of those important dependencies will certainly be induced to infuse a little additional vigor into the works of railway construction, and an increase will be witnessed, in consequence, in the Indian, Canadian, and Australian demand for our railway *materiel*. Not only will such an increase be witnessed, but the development of anything like colonial competition in the matter of railway iron will also become a task of greater difficulty than ever. It is not enough that our ironmasters should now be selling rails at a comparatively cheap rate. They must be able so to sell them, and to still retain a profit upon their industry and enterprise. The question is —"Are the current conditions of the iron trade favorable to the attainment of this result?" We incline to think that they are. Labor is cheaper, coal is cheaper, and foreign iron ore is also becoming more abundant. The very troubles and disasters which have weighed down our iron trade for many weary months appear to have brought with them some compensatory conditions. The cloud of adversity has had a silver lining after all. But still we cannot overlook the fact that our foreign railway iron trade has lost so much ground of late years that even the process of recovery must extend over a considerable period.

RAILWAY NOTES.

PRUSSIAN RAILWAYS.—It is computed that Prussia has now 11,000 miles or thereabouts of railways, which have cost in round figures £310,000,000. In 1870, the amount of capital expended upon Prussian railways did not exceed £125,000,000 so that the additional capital to the amount of £85,000,000 has been laid out during the last five years. It is not to be wondered at, under these circumstances, that Prussian railway enterprise now appears to be flagging. The aggregate receipts have scarcely kept pace with the work of construction.

THE PNEUMATIC RAILWAY IN PARIS.—The "*poste atmosphérique*," for the despatch of messages between Paris and Versailles, has just been completed, and is nearly thirteen miles and a half in length. The tubes, which are made of brass, are 13 ft. long, 3½ in. in diameter, and ⅜ in. thick, are laid at a depth of 8 ft. from the surface, on a flooring of wood, and are pitched both inside and out. The route outside the walls of Paris is along the highway "No. 10." Inside the city they are suspended in the sewers. At the Bridge de l'Alma the tube bifurcates, one portion passing to the Palace de l'Elysee, the other to the Central Telegraph Office. At Versailles the terminus is in the new hall now being built on the left wing of the palace.

AN English paper says: Russian engineers are learning to economize. They are not, as heretofore, sending their old rails to this country as such; but they are keeping them at home, and are sending to this country for iron in a partially manufactured state, with which to utilize the old rails in transforming them into new. Blooms are now being made in the Cleveland district, to be used in tops as single, or as tops and bottoms in double headed rails, in connection with the old rails, which will be employed as the centre of the new rail. The slabs are cut to lengths suitable for the piles built up of the old rails, and they are being made of a quality of iron of extra hardness. Perhaps 600 tons of these slabs are sufficient, with the requisite old rails, to make 2000 tons of new rails. The economy, therefore, which the Russian engineers are adopting, tells upon the demand for rails for Russia, which was previously expressed by existing companies at the iron works of this country.

NEW CAR VENTILATOR.—The Winchell car ventilator, which has been on trial for some time on the Cincinnati, Hamilton and Indianapolis Railway, consists of an air-chamber attached to the roof of the car and extending its entire length. Each end is furnished with a hood, protected by very fine wire gauze screens, through which the air, and nothing else, is admitted to the chamber. Each drum is furnished with a cut-off, operated by a lever within the car, by means of which the supply of air may be regulated. A number of registers in the bottom of the chamber admits the air to the car. When the train is in motion the cut-off in the forward end of the car is opened, and the air enters, passes down through the registers, enters into the car, and, having served its purpose, makes its exit through the rear hood or through the windows, if they are open. In connection with the air-chamber, and for summer use only, are deflectors on the outside of each window, whose purpose is to act as an exhaust, and not only draw out the impure air from the car, but prevent the admission of smoke, dust, cinders, and rain through the open window. These deflectors, which are made of glass, so as not to impede the view from the windows, are operated simultaneously by means of an iron rod running along the side of the car.

NEW RAILMAKING EXPERIMENT. — At the Wyandotte Rolling Mill, at Wyandotte, near Detroit, Michigan, recently, railroad rails were made of iron base and Bessemer steel head, so successfully welded that the most trying tests failed to show even the point of the juncture of the two metals. The same experiments showed the welded rail even better than the solid Bessemer steel. Each was submitted to sixty blows by a 20-ton hammer. The Bessemer rail was completely shattered, cracked through and through in every direction; the welded rail, though mashed down and twisted in the neck, showed no sign of a fracture in any part.

The *Detroit Post* says, this feat was accomplished by the use of a peculiar metalloid sponge or flux in the following manner:—The

pile is made up as for the ordinary iron rail. For the head, scraps of steel, hitherto not used at all, are spread of the desired thickness, and upon this is laid a plate of steel. The flux, in the shape of hard substance about the size of a kernel of corn, is scattered through and over the pile, which is then placed in the oven. When properly heated it is drawn and rolled in the ordinary manner. It is found to be a perfect homogeneous mass; the fibrous iron and the crystalline steel having their particles so interwoven that it is a physicial impossibility to separate the one from the other. Breaking or twisting does not produce a separation at the point of juncture. The wonderful work of uniting iron and steel, so mechanically different, into one homogeneous mass, is accomplished by this peculiar flux, which is composed of fifty-five parts of iron, twenty parts of silicon, and twenty-five parts of aluminium. The chemical action of these substances is explained to be as follows:—The silicon takes up all the alkaline matter, while the aluminium eliminates the free oxygen, phosphorus and sulphur, thus making a uniform, constant, close-grained metal.

ENGINEERING STRUCTURES.

Modern Engineering Feats and Fancies.—The present century has witnessed several remarkable achievements in marine engineering, such as the drainage of extensive arms of the sea in Holland, the construction of the Suez Canal, and the deepening of the estuary of the Mississippi; and, not content with these, still more gigantic schemes have been projected. It has been proposed to admit the Mediterranean into two extensive tracts of the Sahara, which would give water communication to a large portion of Algeria, and make a seaport of Timbuctoo. Neither plan is likely to be put speedily into execution; but in the meantime Mr. Spalding, an American, comes to the front with a proposal to turn the waters of the Black Sea into the Caspian, thus enlarging the latter to its pristine size, and turning the barren and almost impassable deserts left by the subsidence of its waters into a highway of commerce for Central Asia. This ancient sea basin is considerably depressed below the general ocean level, and has been silted up in the course of ages by the Ural, Volga, and other lesser streams which flow into it. The consequence of this contraction and shallowing of the Caspian has been, not only that the land left dry is incurably barren, but that the surrounding country has become unfruitful from want of rain, consequent on the diminished evaporation. Mr. Spalding proposes, as we have said, to restore to the Caspian its ancient body of waters, its ancient depth and area, which was nearly double its present extent, by connecting it with the Black Sea by a channel 150 miles in length, about 170 yards wide at its eastern extremity, but two-thirds narrower on the western half. The projector calculates that at the end of forty years from the beginning of the work, the level of the two seas would be so nearly uniform that the navigation of the new channel could begin. Mr. Spalding further proposes to join the Don to the Volga, and thus lay the sea of Azof also under contribution. The mere excavation of the proposed canal does not seem very difficult, and as the Russian Government appears to have directed its attention to the scheme, doubtless the opinion of competent scientific men as to its feasibility will be obtained. If it should prove successful it would be a magnificent work, and followed by political and economic results more than commensurate with the skill and outlay that would be required for its completion.

The Manora Breakwater, Kurrachee.—The Manora Breakwater was the most important feature of the Kurrachee Harbor Works, which were commenced in 1860, from the designs of the late Mr. James Walker, Past President Inst. C. E., assisted by Mr. William Parkes, M. Inst. C. E. Besides the breakwater the chief works were—in the lower harbor—a stone groyne 8,900 feet long, dredging and removal of rock, and—in the upper harbor—an increase of one-fourth to the area of the backwater, involving a new tidal channel 2½ miles long, crossed by a screw pile bridge 1,200 feet long, and an embankment 2,780 feet long, to close the old channel, also of a jetty 1,400 feet long with quays. All these works are now nearly completed, and have already produced great benefit, the entrance having been made direct instead of circuitous, deepened 6 feet and sheltered, the anchorage space enlarged, and the internal accommodation improved. The trade of the port was 3½ millions sterling per annum, and railway communication with the Punjab would further develop it. About £450,000 had been expended on the whole of the harbor improvements.

The breakwater projected from Manora Point for a length of 1,503 feet into a depth of five fathoms of water, in order to shelter the entrance from the south-west monsoon seas, and to prevent their tearing up sand from the bottom and depositing it as a bar. The characteristics of the sea, wind, and tides, as bearing on the design, were alluded to, and it was stated that the bottom was irregular near the shore. The structure consisted of a base of rubble stone, levelled off generally to 15 feet under low water, and on this base, concrete blocks, each weighing 27 tons, were set on edge, leaning back at a slope of 3 inches to 1 foot, and without bond, two blocks forming the width and three the height, and together making a square of 24 feet in cross sections, the top being about the level of high water. The rubble base was deposited from native boats, and was levelled for the superstructure by helmet divers. Two European mason divers were employed, and six native divers trained on the work, the latter chiefly for shifting the rubble. No accident occurred, and the party generally did not suffer in health. After mentioning circumstances which determined the use of conerete blocks and of Portland cement, particulars were given of the composition of a 27-ton block, the materials being cement, river sand, shingle, and quarry lumps, with saltwater. The ratio of the bulk of the

cement to that of the finished block was nearly 1/3. About 3,500 tons of cement were used, costing from £3 17s. to £4 10s. per ton. The mixing station, block ground, and moulding of the nineteen hundred and seventy-two blocks, including three hundred and twenty-five of special smaller sizes, were then described; and it was remarked that the "Messent" mixers had been found very efficient. The blocks were sometimes used one month after being made, and once, as an experiment, a 27-ton block was safely lifted in seven days. When the work was fairly established the blocks cost for current expenses 15s. per cubic yard, though the average total rate was raised beyond this by extra expenses in the earlier stages.

The blocks were lifted on to the trucks by a steam hydraulic traveling crane of 50-feet span; each truck carried one block, and was taken separately by a tank locomotive to the breakwater. The blocks were set by a steam traveling crane, called the "Titan," which ran on rails laid on the finished work, and overhung the end, so as to carry the blocks of three tiers in advance to their places, thus dispensing with staging. The framing of this crane supported a traveler and crab, worked by an eight-horse power engine on the top, which also drove the traveling gear of the entire machine. The cost of the Titan, delivered and erected at Kurrachee, was £2,879. The rate of setting was limited by the progress of the foundation and by the supply of blocks, but during the last season ten 27-ton blocks were set daily on an average, while on one occasion six blocks were laid in one hour and forty minutes without special pressure.

The base was commenced on March 17, 1869; and later in that year the shore end "stump," 45 feet long, to make a starting place for the Titan, with other preparatory works, was completed, after some unavoidable delays, and the first block was set on November 1, 1870. The delays of the foundation were specially felt in the first season's work, but a length of 225 feet was built in four months, taking the breakwater out to 270 feet from the shore. During the second season, 1871-72, after a few days spent in repair of monsoon damages, a length of 523 feet was built in about four months, making a total of 793 feet. During the third season, after the repair of monsoon damages, a length of 710 feet was built, completing the breakwater on February 22, 1873, to its full length of 1,503 feet, which had thus been barely twelve months in actual building.

The action and effect of the monsoon sea, and the repair of damages were then detailed. In 1871 the center joint opened here and there, and one block was washed over from the top course on the harbor side. Slight damage also occurred to the shore end in the sea angle. The nature of the settlement was described, also a curious rocking action, and the closing up of the cross joints under the action of the sea. The repairs of the damage, in the first season, cost £185. During the second monsoon, 1872, twenty-five blocks were washed out from the top course on the harbor side, eighteen of these blocks being in one length of 86 feet. The damage was again traceable to inequality of settlement. The sea side did not suffer, nor did the shore end, though both showed evidence of the force of the sea. The damage was repaired in a few days at a cost of £512. The monsoon of 1873, the first after the completion of the breakwater, did trifling damage, and was confined to the shore half length, still pointing clearly to weakness of foundation. The repairs cost £199. In the monsoon of 1874, the outer end and "scar," which had not then been in any way specially secured lost five blocks during unusual weather, though no other part of the outer half length suffered, but the shoreward half opened here and there. The repairs of this season cost £418 and included the re-erection of an iron beacon on the outer end. The nature and extent of the subsidence (which in some parts amounted to 3 feet, but without dislocation), were then noticed, and the action of a mollusk, the "Pholas," on the concrete blocks, also the effects of the sea on the rubble base, which did not, however, affect the stability of the superstructure.

The cost of the breakwater had been £93,565 or £62 5s. per lineal foot, but this amount included preliminary charges, the current expenses during the last season being only £34 per foot. This sum included the repair of damages during the progress of the work, and during the two monsoons since its completion, but not the expense of engineering and office establishment. The work had been carried on in the Bombay Public Works Department by the Author and his assistants, advised by Mr. William Parkes, M. Inst. C. E., as Consulting Engineer, and without the employment of any general contractor. The completion of the work was favorably noticed by all the Government authorities concerned.

ORDNANCE AND NAVAL.

IRON SHIP CONSTRUCTION.—The following is the substance of the paper read by Mr. De Rusett at the late meeting of the Society of Naval Architects, relative to the longitudinal girder or bulkhead system of iron ship construction patented by Messrs. C. Chapman and E. W. De Rusett.

The reader of the paper said the patentees attributed some of the calamities to long steamers to weakness in construction, especially as the losses of unclassed vessels are nearly twice as numerous as those which are classed, and they say, "our opinion is confirmed when we have the results of the elaborate calculations of Mr. Johns before us, showing that first-class vessels of 3000 tons and upwards, built to Lloyd's rules of two or three years back, when poised on the crest of a wave equal to the vessel's length, have only a margin of safety of 2½, which is not half of that considered necessary for structures on land, such as bridges, &c., which have no such variable loads as a ship has to bear, neither are they subject to the sudden additional strains of immense masses of water heaped on them, or the violent blows from giant ocean waves, or the terrible vibration of the huge screw when the vessel pitches heavily before an Atlantic swell." The patentees endeavor to show that a vessel

may be so constructed that she would be, as compared with others now in use, nearly unsinkable; and in order to make it clear that such a vessel can be constructed it will be necessary, in the first place, to show how a greater strength can be obtained without making the vessel more costly and heavier. "An iron vessel of 3000 tons is only a quarter as strong as an iron vessel of the ordinary kind whose tonnage is 100, and a vessel of 3000 tons is only half as strong as a vessel of 1000 tons. The above are mentioned as examples to set people thinking about the respective strength of large and small vessels; and having given a little thought to the matter, they will naturally say to themselves, 'Why are the large vessels not built stronger?' The answer to that is that they cannot build them much stronger without making them so heavy and so costly that, commercially speaking, they would be failures—that is to say, if they continue to build them on the present principle of construction. Therefore, before a vesssel can be built at anything like the same price to be very much stronger, and without additional weight, something new must be added to her in some particular part of her that will give greater strength—in fact it must be put in such a position in the vessel that it will admit of the taking away of great weights from other parts, so as to bring the weight of the vessel to what it would have been had she been built in the ordinary manner." The plan submitted to the meeting was as follows:

"We lay the keel of the ship down exactly as if were going to build an ordinary vessel. After the keel is laid, we place a sheet of iron on one side of it—say the port side—and this sheet runs down to the bottom of the keel, and is drilled for riveting to it. This sheeet of iron runs right fore and aft the keel, and is riveted to both the stem and the stern post, and rises perpendicularly, right in the midships of the vessel. The first plate of this longitudinal bulkhead having been placed by the side of the keel, both sides of the vessel's bottom may be placed just the same as if there had been no alteration in the construction of the vessel whatever. With respect to the shape of the vessel's bottom, there need not be the slightest alteration in it, nor need there be any alteration in the weight and strength. After dividing the vessel into two parts by means of the longitudinal bulkhead, the vessel is again subdivided into nine compartments, which give the vessel in all eighteen compartments, and each of these latter is really strong enough to hold on against the pressure of the water—they are, in fact, as strong as any other part of the ship. Therefore, assuming that four vessels ran into her at one time, say two on each side, she will not sink, not even if she gets the hardest knocks that one ship gives to another under such circumstances. There is the same degree of safety to life when this type of ship gets on shore; it will take four times the strength or force of the ocean waves to break this vessel up than it would to break up an ordinary vessel. Again, should a fire break out on board of this class of ship, any of the compartments where the fire was raging might be at once flooded without the slightest danger of sinking the ship."

The patentees advocate twin screw propulsion, as advocated by Capt. T. E. Symonds in 1862, and others, and now generally adopted by Her Majesty's Government in the largest ironclads, as well as gunboats, and applied to merchant ships. Among the former may be mentioned the *Invincible*, *Iron Duke* and the *Vanguard*, all vessels of 6000 tons and about 4800 indicated hore-power; and also the *Thunderer*.

AUSTRIAN IRONCLADS.*—Having recently had occasion to ask my friend, Herr Romako, the chief constructor of the Imperial Austrian Navy, for some particulars of certain ironclad vessels designed by him, I have thought it would be useful to the members of this institution if I placed an outline of the same before them.

The first ship which I propose to mention is the new ironclad *Tegetthoff*, now under construction at the works of the Stabilimento Technico, at Trieste. This large establishment took the contract for the hull, engines and boilers in August last. The following figures give her dimensions, calculated elements, &c. Length between the perpendiculars, 286 feet 11¼ inches; length, total, 303 feet 1½ inches; breadth on the water-line, 62 feet 9 inches; extreme breadth to the outside of armor, 71 feet 1½ inches; depth of hold, 34 feet 9 inches; draught of water, aft, 26 feet 7½ inches, draught of water, forward, 23 feet 1 inch; displacement with the half of provisions, 7390 tons; area of the midship section, 1301 square feet; area of the load water-line, 14,308 square feet; height of metacenter above center of gravity of displacement, 14.623 feet; height of metacenter above water, 4.770 feet; distance of the center of gravity of displacement before the midship section, 3.356 feet; depth of the centre of gravity of displacement below water, 9.853 feet; coefficient of displacement, 0,582 feet; co-efficient of water-line, 0.782 feet; co-efficient of midship section, 0.82 feet; displacement of an inch immersion at the load water-line, 34.47 tons; weight of armor and backing, 2160 tons; the armament consists of six 11-inch Krupp guns. Area of sails, 12,165 square feet; cost of hull, estimated, £172,790; cost of engines and boilers, estimated, £81,715; nominal horse-power, 1200; number of cylinders, two; diameter of cylinder effective, 125 inches; length of stroke, 4 feet 3 inches; Griffiths propeller, diameter 23 feet 6 inches; pitch 24 feet; number of blades, two; revolutions per minute, seventy; number of boilers, four; area of fire-grate, 850 square feet; heating surface, 25,500 square feet; superheating surface, 1800 square feet; pressure of steam, 30 lbs.; number of furnaces, thirty-six; mean indicated horse-power, 8000; speed, estimated, fourteen knots.

From these figures it will be seen that although we are not dealing with a ship of the *Inflexible* (English) or of the *Dandolo* (Italian)

* Mr. E. J. Reed, before Naval Architects.

type, in which armor of excessive thickness is placed over a central citadel of extremely limited extent, we, nevertheless, have a very powerful ship indeed, with armor of apparently about 13 inches to 14 inches thick, and with a concentrated battery of six 11-inch Krupp guns, each weighing, I presume, about 27 tons. The ship has a belt of armor extending from the stern to within about 30 feet of the foremost perpendicular, where it terminates in a transverse armored bulkhead, and a stout iron deck going forward to the stem at about 7 feet below water.

In this ship the Austrian Admiralty have adopted an improvement in armor to which I have for a long time past attached great importance—I refer to the getting rid for the most part of great curvature. Of course armor-plates, if carried round the ends of a ship, must be bent to the curvature of the water-lines, and when embedded so to speak, in the sides of a ship of ordinary form and curvature—as has been usual in sea-going ships—they must also be bent crosswise. Now this double curvature of the plates is not only an expensive process, but it is also injurious to the armor-plates in some degree. To foreign Governments which do not make their own armor-plates, and especially to the Governments of countries very remote from England, the system will further be attended by extreme difficulty in replacing plates injured in battle. This could hardly be accomplished by the slow and expensive process of sending accurate moulds to England for the guidance of the manufacturers. By so designing the ships that the armor-plates have only to be curved in one direction, all these disadvantages and difficulties are practically got rid of, and therefore, in recent ships which I have had occasion to design for foreign Powers, I have carried out this principle, and Herr Romako, in a letter to me, says:—"With your encouragement I have undertaken to give the stern a form which enables the bending of the plates to be performed in only one way."

The next feature to be remarked in this ship is that the battery is of the projecting type, which so greatly facilitates the obtainment of direct fire ahead and astern from a midship battery without excessive recession of the unarmored parts of the ship before and abaft the battery. The Admiralty constructors and myself introduced this arrangement in many cases of upper-deck batteries in ships designed while I was at the Admiralty (most notably in the case of the *Audacious* class), but I do not think the same thing has been done at the Admiralty in the case of the main-deck battery. I have, however, done it myself in several ships for foreign Governments, such as the *Kaiser* and *Deutschland* (German), and the *Almiranti Cochrane* and *Valparaiso* (Chilian) armor clad ships. In the *Tegetthoff* the overhang is very low, and considerable in amount, the battery projecting between 4 feet and 5 feet, the spread commencing at 18 inches above the water, and terminating at a height of 6 feet.

A still more novel feature, one which, although I have seen it suggested before, has never before, to my knowledge, been carried out. It consists in depressing the sides of the ship into curved indentations in wake of the guns. The object mentioned to me by Herr Romako, as that which has been sought in the arrangement, is the protection of the gun-muzzles. In mentioning the general form of the battery, and more particularly this feature, he says: "The ship *Tegetthoff* is in many regards a novelty, its casement allowing an all-round fire, avoiding at the same time, by its particular form, the dangerous projection of the muzzle of the midship guns, in consequence of experiences acquired in the battle of Lissa, but which are very little known even in our own navy."

The *Tegetthoff* is also to be furnished with a transverse bulkhead abaft the foremost gun, an arrangement which will prevent the battery from being raked in chasing. This improvement exists in the *Alexandra*, where it was, I believe, introduced for the first time by the present Admiralty constructors. The foremost bulkhead of the battery is inclined forward at a considerable angle to within about 4 feet of the middle line, where it becomes transverse. Immediately over this foremost portion of the battery at the middle is a very strong pilot tower, standing well up above both the gunwale and the forecastle. This shows that the Austrian officers, who have been in an action with ironclads, do not consider such towers unnecessary.

It may be interesting to add that while the outer skin and angle irons of the hull are of iron, all the remainder is of Bessemer steel, varying in tensile strength from 30 to 33 tons per square inch of section, and possessing this, as I am informed, in combination with 25 per cent. of ductility. This Bessemer steel is produced very successfully in Styria and Carinthia, from which districts of Austria the chief supplies for the *Tegetthoff* are derived.

BOOK NOTICES.

THE THEORY OF SCREWS. By ROBERT STAWELL BALL, LL.D. Dublin: Hodges, Foster & Co. For sale by D. Van Nostrand. Price $5.25.

The sub-title to this work is, "A study in the dynamics of a rigid body." This explains the scope of the work much better than the first one.

Students who are fond of working in the field of the higher theoretical mechanics will read the work with satisfaction, but no others will.

POCKET-BOOK OF USEFUL FORMULÆ. By G. L. MOLESWORTH. London & New York: E. & F. N. Spon. For sale by Van Nostrand. Price $2.00.

This is the eighteenth edition of this compact and convenient little pocket-book. It has been gradually enlarged in the later editions, and the older portions improved by the elimination of the errors of typography, which are incidental to first editions.

The present edition is more than double the size of the first one. It has been increased from 230 to 592 pages.

Among the later additions, we notice a short synopsis of methods of the calculus, and some thirty pages devoted to telegraphic engineering.

A Short History of Natural Science. By Arabella B. Buckley. New York: D. Appleton & Co. For sale by D. Van Nostrand. Price $2 00.

If this book is what it appears to be to such brief inspection as we have been able to subject it, it deserves a place in every library, and is indispensable to every school library.

A history of the rise and progress of scientific labor, with plain descriptions of early methods of research, and carefully compiled chronological tables, is certainly something desirable to have, and is nowhere else attainable that we know of.

The style is agreeable, and the illustrations, though few, are well chosen. Although written for schools and young persons, old persons out of school will read it with pleasure and profit.

The Art of Furnishing on Rational and Æsthetic Principles. By H. J. C. London: Henry S. King & Co. For sale by D. Van Nostrand. Price $1.75.

The subject upon which this little book treats has lately attracted much attention. There is abundant evidence in the new forms of furniture that the popular interest in the matter of tasteful home decorations is steadily increasing.

Mr. Eastlake's "Hints on Household Taste" did, and is yet doing, good service. The present volume is much less pretentious, and affords general rather than precise hints to the reader. It is divided into two parts; the first treating of Decorations, the second of Furniture, and each part is divided into chapters, devoted separately to the hall, dining-room, drawing-room, library, etc., etc.

The book is easy reading, without illustrations, and we doubt not will prove widely useful.

The Sanitary Drainage of Houses and Towns. By George E. Waring, Jr. New York: Hurd & Houghton. For sale by D. Van Nostrand. Price $2.00.

This work is designed to diffuse a better general knowledge of the subject of drainage than can be obtained from any other than professional works. It is free from technical discussions, but covers the ground quite completely.

It contains just the knowledge needed by the young engineer who must know the general principles which underlie his future practice.

It is calculated, moreover, to stimulate town authorities to a better performance of general sanitary work, by clearly setting forth their duties.

The contents herewith appended exhibit the range of topics:

The Sanitary Relations of Drainage; The Drainage of Houses; The Drainage of Towns; Arranging Plans for Town Sewerage; The Construction of Sewers; The Details of House Draining; The Dry Conservancy System; Vaults and Privies; Liernur's Pneumatic System; The Disposal of Sewage.

Thirty-one woodcuts illustrate the text.

Design and Construction of Dock Walls. By J. Romilly Allen. London and New York: E. & F. N. Spon. For sale by D. Van Nostrand. Price $2.50.

The present work covers only the subject of *designing* dock walls; the second portion, soon to follow, will be devoted to the more practical part of construction.

The author has logically developed his subject, beginning with the considerations of water and earth pressures against a plane surface; he considers next the statical conditions of the wall, and then the direction of the resultant of these forces. Then come walls of different sections, with and without a *surcharge*.

The mathematical work is of an easy kind, and the solutions are to a certain extent graphic in their character.

The subject is an important one, and is clearly enough treated in the present work, although all the material could have been put quite as satisfactorily in a much smaller space.

The typography and diagrams are excellent.

Naval Powers and their Policy. By John C. Paget. London: Longmans & Co. For sale by D. Van Nostrand. Price $5.25.

The principal portion of this work appeared originally in the *St. James' Magazine;* some additions in the way of statistical information have been made to the first articles.

The work is not so much an essay as a comparative statement of the naval armaments of British and foreign ironclad navies.

The table of contents sets forth the following list of subjects for the separate chapters:

Recent Growth of Foreign Navies; Naval Policy; Strength and Distribution of the British Navy; Personnel of the British Navy; Finance; Naval Volunteers and Coast Defence; Our Ironclads; the Inflexible; The French Navy; The Austrian Navy; The Turkish Navy; The German Navy; The Italian Navy; The Spanish Navy; The Russian Navy; The Scandinavian Powers; The Navy of Holland; The Navy of Greece; Brazil, Chili, Peru, Argentine Confederation; Naval Guns; The American Navy; Suggestions; Tables.

The volume is exceedingly well printed. No cuts are given, as the plan of the work requires none.

Railway Appliances. By John Wolfe Barry. London: Longmans, Green & Co. For sale by D. Van Nostrand. Price $1.50.

This book is the latest issue of the series of Text Books of Science, and is in no way inferior to its predecessors. It is essentially a work for the practical railway engineer. It deals with the lesser details of construction, and with systems of management.

Under the head of "Permanent Way" are described various forms of rails, rail joints, sleepers, chairs and their fastenings, and the action of the wheels on the rails.

Under "Points and Crossings" are treated Safety-points, three throw-points, single-tongued points, slip points, and various forms of crossings.

"Signals" deals with all the various forms of hand, mechanical and electrical signals.

One chapter is devoted to the "Block System," another to "Stations" and includes such adjuncts as turntables, weighing machines, etc. "Rolling Stock" is a comprehensive subject and occupies corresponding space. Seventy pages are devoted to minute descriptions of cars, wheels and brakes.

Nothing relating to English Railways is left undescribed. Nothing could be better apparently than the methods of description. Over two hundred wood cuts are distributed throughout the book.

MISCELLANEOUS.

Of the 50,707,175 tons of coal mined in the United States last year, Pennsylvania produced 36,547,615 tons, or 72 per cent. of the whole output.

We learn that the sub-Wealden bore is temporarily arrested at 1672 ft. from increasing deposit from the sandy beds. The original problem was dependent on the opinion of geologists that palæozoic rocks would be found at a depth varying from 700 ft. to 1700 ft. So far, however, the strata are mesozoic; but the latest fossils give some indications of a palæozoic rock. Much hope is therefore entertained of solving the problem.

Improvements in Safety-Lamps.—The invention of Mr. A. B. Boullenot, of Paris, consists in replacing the Davy or safety-lamps ordinarily used in mines containing fire-damp by improved lamps supplied with air from outside the mine. For this purpose a fixed pipe or pipes is conducted down the mine, and branches from it are led into all the workings. Compressed air is forced down the pipe by means of air-pumps worked at the surface, and the improved lamps are screwed to the air-pipes where necessary by means of couplings and straight or elbow pipes provided with stop-cocks. The improved lamp consists, first, of a metal air vessel which receives the compressed air, and into the upper part of which an oil vessel drops, fitted with a burner, and a wick (either flat or tubular) which can be raised or lowered by means of a knob outside the lamp. This knob can be disconnected from the oil vessel when it is required to remove the latter. Above the air vessel is cemented a tube or cylinder of glass or crystal, the upper end of which has fitted round it a metal ring carrying a cover formed of two pieces of wire gauze a short distance apart. The glass cylinder is protected by a cage of sufficiently strong metal bars. A reflector is provided carried by a ring fitting round the air chamber and movable into any desired position. Through the center of the gauze cover an opening is left which can be closed by a stopper, and through this opening a light can be introduced consisting of a tube or sheath containing an inner spring clip, which can be raised or lowered by a ring or handle at the top of the tube from outside the lamp. This spring clip carries a match, which can be ignited by rubbing its end upon a rough surface prepared for the purpose at the top of the oil vessel. When the match is ignited the lamp wick is lighted from it, and the match is then extinguished by drawing the spring clip into the tube, which can then be removed from the lamp, the opening in the cover being closed by its stopper; and the lamp will then continue burning so long as the supply of oil and air continues. A valve may be provided in the cover. When necessary this improved lamp may be screwed to a portable receiver of compressed air or oxygen, instead of to a fixed air-pipe.

An Innovation in Roof Construction.—At a meeting of the Manchester Scientific and Mechanical Society, Mr. Grierson in the chair, Mr. E. Grimes read a paper on the above subject.

The author, in the course of his paper, said, recently he had observed that designers of roofs, both architects and engineers, had departed from the old plan of fixing purlins with their upper surfaces parallel with the plane of the roof, and their sides perpendicular to the same, and, instead thereof, had placed them in a position with their sides vertical and their upper surfaces level. He was not quite sure, but he had reason to believe, that engineers first adopted this arrangement and that architects followed suit, presuming, doubtless, that any change made by engineers, who were usually supposed to possess superior mathematical knowledge, could scarcely fail to be right. In his opinion, however, this new mode of construction was incorrect; and, indeed, it would require very little consideration of the forces brought into action in constructing a purlined roof to demonstrate that the practice universally adopted from time immemorial was the mechanically correct one. His attention was attracted to the new arrangement and its results on the occasion of a visit to the Birkenhead Docks. Passing through one of the large coal sheds he observed that the purlins of the roof, which were of timber, (although the principals were of iron), and of great length, were trussed with iron rods and placed vertically. These were in many cases bent considerably by the weight of the roof coming in a horizontal direction, and one or two of them were so much bent that the middle portion of the trussing rods which had been upset and hollowed to form a seat for a large plate stud, were forced aside from their place on the stud to a considerable distance. Looking on the outside of the roof it was at once apparent that there it had sagged, as they termed it, and was a hollow plane. Since that time he had observed several roofs having vertical purlins, and presenting similar results. It appeared to him that the error had arisen from supposing that gravity was the only force necessary to be resisted, in which case, doubtless, the placing a purlin in a vertical position would be the strongest way. This, he contended, was not the fact, the force opposed to gravity coming into play and necessitating the strongest section of the purlin to be placed in a line with the resultant, and perpendicular to the plane of the roof.

The reading of the paper was followed by a discussion in which a general concurrence was expressed in the conclusions arrived at by Mr. Grimes.